

# M I N D

## A QUARTERLY REVIEW

### OF

## PSYCHOLOGY AND PHILOSOPHY



### I.—OTHER MINDS (VII.).

BY JOHN WISDOM.

*Black.* "Are immortal!"

*White.* Yes, *are* immortal. A flash reveals that the road we are on leads to Solipsism. Elijah mounts the flaming car and at that instant, in that moment, gains immortality; and we begin to see how in all the present is hidden the future, from the bird always renewed to the flower that fades so fast.

"Is now immortal", "is deadly", "has a soporific power", "is a poppy", "is a silver rose upon an azure field"—each of these is a claim about what the nature of something is at the moment the speaker speaks, yet each makes a claim about the future. Even if the exclamation "A rose on azure" is used as no more than a description of a pattern we can see and makes no claim as to whether what we see is a flower we can pluck or a sign in heaven we can never reach, still it makes a claim about the future. For whatever you may see above there's no rainbow there if no one sees it too.

Of course there's a difference between a pattern and a poison. "It's poison" obviously makes a claim about the future in spite of its present tense, "Poppies" less obviously, "A halo" or "A rainbow" (purely visual) still less obviously. For a rainbow carries its character in its face and the more a thing does that the less does anyone who tells us that it's here make any claim about the future.

*Gray.* But surely the more a man's face tells us of its character the more it tells us of the future.

*White.* But a rainbow's face doesn't tell you of its character, it *is* its character. A man's face may tell you about his character and tell you wrong. A rainbow's face never does, never can. But this isn't because Iris is so ingenuous. However ingenuous she were she might at any time be tempted to be disingenuous. No, a rainbow's face can not mislead as to its character for this best of reasons—a rainbow has no character at all or if you like its character is its face.

*Black.* Surely what you are *now* saying is that "There is a rainbow (purely visual) in the sky" does *not* make any claim about the future. What did you mean when you said that it does? Of course if we take a rainbow as a token of a covenant, so that a coloured bow is not a rainbow unless there is sunshine to follow, then "There's a rainbow" does involve the future. But then its character isn't its face. If "There's a rainbow" is taken as you have reminded us it may be taken, namely, as a statement that there's a many coloured bow-pattern in the sky, then surely it becomes a statement purely about the present and not involving the future.

*White.* *Relatively* it is a statement about the present. I mean that in the ordinary way we should say that "There's a rainbow" is a statement about the present, and we should also say that it is categorical and in these ways oppose it to such statements as "There will be a rainbow in about five minutes' time" and "You will see a rainbow in five minutes' time" and "If you look out of the window in five minutes' time you will see a rainbow". Even when we have considered such statements as "That's poison", "That's the best steel", "Jones is very strong", and decided to say that these are really about the future and are really hypothetical, we may still insist that "There's a rainbow" is really about the present and categorical, especially, very especially, when we have explained that we are thinking of a use in which "There's a rainbow", unlike "That's arsenic" or "That's cheese" involves no claims about the reactions of rats, cameras, dictaphones, or even about what we shall smell or taste, that is, is about an utterly ineffective and purely visual object.<sup>1</sup>

In spite of all this it is quite possible to think of statements which *still less* involve anything about the future, and then one begins to say of even the purely visual use of "A rainbow!" that really it involves a claim about the future. And this inclination is increased when one sets out those features of its

<sup>1</sup> Compare 'is poisonous' is a dispositional property but 'round' and 'sweet' and 'red' are not and can be detected in *one* glance.

use which make it different from those other, even more cautious statements, about which one feels, "Now these really are statements about the present, categorical, and such that one may know them to be true".

We have taken a use of "There's a rainbow" which involves no claim about what cameras or other instruments will show. You now ask, "Isn't this a statement about the present?" Now even when only purely visual rainbows are in question one may very well say, "There must be a rainbow", or "There must be a rainbow somewhere", even when no one sees a rainbow. It is at once clear that this statement makes a prediction. For if someone looks and sees no rainbow he will say, "No, you are wrong. You look. There's no rainbow." It is clear that this objectivity makes the statement involve a prediction. For it now involves that anyone who looks will see a rainbow. Of course this must not be taken to mean that if Jack looks and fails to see one then there isn't one there. If one thing is heavier than another then that involves that if we try them on a pair of scales the one will tip the other but if this doesn't happen when they are put on my scales it doesn't follow that the one isn't heavier than another. There may be something wrong with my scales. There may be something wrong with Jack. "It's a cow" involves that it chews the cud but, of course, if it doesn't, then, provided it gives milk, is born of cows and bears cows, it's a cow which doesn't chew the cud. Probably there's something wrong with its internal structure. But possibly not—you never know—and even if there isn't it's still a cow. Here appears again how every fact is a matter of an infinite pattern of facts fading but never disappearing in the distances of Space and Time.<sup>1</sup>

*Black.* But surely the *present* existence of the rainbow isn't constituted by its being seen or seeable in the future.

*White.* Is the actual and present poisonousness of arsenic something different from the hypothetical fact about the future that if an animal takes it it will die? Is the immortality which Elijah has now gained anything more than the fact that he will not die? Is the weight of a cake or a feather, a liquid hidden within it, like water in a sponge, which will cause it to depress scales in the future? Isn't it rather that the cake's being heavy, as opposed to the sponge's being heavy, just is a matter of its depressing scales and giving a feeling of tension in the muscles of those who lift it, *i.e.* feeling heavy to those who lift it? And

<sup>1</sup> See R. B. Braithwaite, "Propositions about Material Objects", *Proc. Aris. Soc.*, XXXVIII.

isn't the softness of a feather that sort of thing too? And its whiteness.<sup>1</sup> Indeed the whole feather? And is our rainbow any less a matter of what people will see if . . .

It sounds queer to say that the giving of a gift involves the future. If you give a man a gift of a thousand pounds at two o'clock on Tuesday do you give it him then or not till later? For "after all", and "if you come to think of it," as the philosophers say, what you really give him is nothing but a piece of paper—a cheque,—and if it isn't honoured you didn't give him a thousand pounds, only a worthless bit of paper. Every statement is a cheque drawn on the bank of Time.

*Black.* But take the case of a man who while actually seeing a rainbow exclaims, "A rainbow". Surely then his statement involves nothing about the future.

*White.* It involves nothing about the future only if it is used to describe only what he can see, in other words his own sensation at the moment. Otherwise his statement still involves claims about what others will see if they look. In other words what makes the statement objective also makes it predictive.

*Black.* Suppose that everyone who hears him is actually seeing a rainbow as the speaker speaks and that sudden and inexplicable disappearances are the usual thing with rainbows so that what they see or fail to see a moment later counts not at all against their statement, "A rainbow".

*White.* Notice how in so far as for each man the statement is less about the future it is less informative. That, however, is not the point at the moment. The point is that for each hearer in so far as the statement is objective for him that far it is predictive for him. The statement is objective for each hearer while in accepting it he is prepared for the echoes in others of his sensations of a rainbow; I mean that when he asks others, "Do you see a rainbow?" then he expects them to answer "Yes". The statement would be subjective for him if you took it as you would in the following case: a doctor gives you a dose of a new drug and then says, "A purple circle, a rose on azure, a rainbow round a railway train bound for the Golden Gate", and to your astonishment he thereby describes very well the sequence of your images. His statements involve for you no predictions and therefore no predictions which for all you really know just conceivably might not be fulfilled. You

<sup>1</sup> Whiteness and softness can be felt in a moment. But so can immortality. Maybe behind the celestial horses Elijah *felt* immortal all the way to Heaven but so no doubt did Mr. Walter Winans on Brighton front behind a pair of trotters.



know his statements to be right. But then they are just statements about what you are seeing, about your sensations at the moment he speaks. And it is precisely because they are just that that they involve no prediction and thus are known, not from the smell, not from the present, not *from* anything, but completely and thus absolutely directly, and it is just because they are known directly and without any "knowing" what will happen *from* what is happening, that they are known without the possibility of error and thus really known.

*Black.* You mean that unless the hearer knows that others are seeing a rainbow then even if he is seeing one he doesn't know whether the statement, "A rainbow", is correct unless he takes that statement as descriptive merely of his own sensation. But imagine that all the hearers are also speakers, that everyone sees a rainbow and that everyone shouts "A rainbow" and hears everyone else shout it. Surely there is then no prediction and no possibility of error in their statements although the statements are still objective.

*White.* Given that the statement, "A rainbow", is used in the way described and that it is uttered by everyone which its use makes relevant and that each of these is seeing a rainbow then nothing in the future can make that statement false. But it is not true that each speaker is given all this. He just sees as it were a rainbow and hears as it were shouts. So that it remains true that the shout, "A rainbow", involves for each hearer no prediction only in so far as it is descriptive for him merely of his own sensations of the moment. Does he count anything else or doesn't he? If he doesn't then the shout "A rainbow" amounts for him to "It will seem to you just as if there is a rainbow and just as if everybody is shouting 'A rainbow'". If he does count something else, such as whether people are really shouting or really seeing what they say they see then "A rainbow" means to him something real, outside his mind, and is not merely the description of the moment. But then what would this something else counting be? What, for example, is the difference between a man for whom it matters whether people really saw what they said they saw and one for whom this doesn't matter? The difference is this sort of thing. Suppose that most of those who shouted "A rainbow" subsequently confess that they saw no such thing. When this happens one of those who saw a rainbow says, "So there wasn't a rainbow after all. You were all lying. You were all pretending you could see a rainbow", and turning to his doctor friend he says, "Jack, you said there was a rainbow, but it wasn't true,

it wasn't real, it was only another illusion ". This is the man for whom something else counts. Another man says, " No. All they said was true. For it meant to me just the colour and the shouting. That was there and can't be taken from me, and when like the rest I shouted " A rainbow " I was right for that is all I meant, that there was a rainbow for me, that come what might it was a rainbow moment. For this man nothing counts but his own vision of a rainbow and his own hearing of sympathetic shouts. The first man was one for whom not merely his own sensations but also something else counted. And we say this because he counted the confession he *subsequently* heard.

Take another case. Suppose " A rainbow " means nothing to a man about what cameras or even other people see. It may still be more for him than a description of his own sensations of the moment. Suppose that he has been suffering from waking nightmares or that there are four or five waking nightmares from which people suffer. For each nightmare there is a dominant hallucination which appears in all its changing scenes, for one nightmare a rainbow, for another a dagger, and so on. Characteristic feelings accompany the varying patterns of scenes. Then when a man says " A rainbow ", he will say this because he sees a rainbow and takes this as the precursor of a well-known pattern of hallucinations. And he and his friends may come to use the sentence, " A rainbow ", in such a way that if the well-known pattern fails to follow they say, " No, I was wrong, it was only a passing vision " as an alternative to " So it wasn't real *rainbowenia* ". Under these circumstances the words " A rainbow " have come to mean to each of them much more than a temporary sensation of a rainbow. True, the words no longer mean to the person to whom they are addressed anything about what others may see and mean only something about what he will see. In that way they are subjective. But they are much more to him than a description of what seems to him to be so at the moment and in that way they are objective. Of course we can again reduce the claims about the future and so reduce the risks. But again the risks don't vanish till the claims have vanished too. Only if " You have a cold " means merely " You feel now just as if you have a cold ", do the feelings you have in your nose and throat suffice to decide whether the statement is correct or incorrect. They don't suffice to determine whether you really have a cold. And the logic of love is the same—of real love, real hate, real understanding and real doubt. A psychological reality is an illusion

that lasts. In other words : Statements based on introspection, statements about one's own state of mind, when they are not descriptive merely of one's immediate experiences, differ from statements based on perception, statements about material things, only in the sort of subsequent pattern that is relevant to them.<sup>1</sup>

*Gray.* But is *every* statement which goes beyond the sensations of the moment predictive ? And in particular must every statement about the mind of another, even about what *sensations* that other person is having *at the time*, mean to a hearer something about the future ? Does "Smith is now in pain" mean to Jones something about the future ?

*White.* The actual present weight of a thing is really no more than the fact that it will tip scales and cause certain sensations and so on. Opium is an anodyne and its being so is no more than the fact that it will stop pain. Arsenic is the opposite and its being so is no more than the fact that it will start pain. The luminosity of paint is its looking white in the dark, and the greenness of a frock is its looking green by day and not merely in the light by which the assistant is showing it to you. The greenness of a thing and one's having a cold, in so far as they are not merely matters of a thing's seeming to one at the moment green or snuffly, involve what will happen or what would happen if . . . And for you, in so far as Smith's pain is not merely his seeming to you now to be in pain, that far Smith's pain for you involves what will happen or what would happen if . . .

We have seen this already. For in passing from a real, physical, material rainbow to a private, mental rainbow, we on the way considered the change from a non-physical and purely visual but still public rainbow into a purely private rainbow. In doing this we set out what changes a statement about what others see into a statement which is merely about what one oneself sees. For to take the statement, "A rainbow", as one about a public, objective though not physical thing is to count against it the failure of others to see any such thing, while to take it as describing a private rainbow is to count only one's own sensations at the moment. And the changes from the statement about a public rainbow to one about a private rainbow, like all the others on this route to security, consisted in the reduction of claims about the future and in this instance to the reduction of such claims to zero. The reverse of the change is the restoration of those claims.

*Gray.* I feel that there's trickery here and that you have

<sup>1</sup> MIND, Vol. L, N.S. 200, especially p. 322.

slyly introduced Materialism and Behaviourism under the cloak of Idealism, Sensationism and Phenomenalism. I feel that you begged the question by slyly asking, "Surely what makes for Jones the statement, 'A rainbow', one about a public rainbow and not merely about his own sensations, is his counting against it any signs he may subsequently find of others not having seen what he saw?" The argument runs: And what do you mean by Smith's pain *for me*? And anyway, Smith's pain for him isn't merely a matter of the future. So how *can* it be for me?

*White.* Was what we said true? A circular proof is a valid proof. And it is not futile provided it is sly. True, a demonstrative proof decides nothing. The old difficulties about the conclusion will be there still. And the better the proof the more they will reflect on the premisses. But demonstrative proof is not therefore futile. It musters for us those things which tend to make us accept the conclusion and forces us to bring out those things which tend to make us refuse the conclusion and therefore the premisses.

However, the immediate issue is not Behaviourism but Predictionism, the doctrine which Professor C. I. Lewis so well sets out in a passage including the words, "Is it not the case that the simplest statement of objective particular fact implicitly asserts something about possible experience throughout all future time . . . ?"<sup>1</sup> The subtle and conflicting connexions between what we have been saying and Behaviourism we must look into later. But the immediate issue is: Is it true that for Jones even such a statement as "Smith is now in pain" really involves a claim about the future?

As I said, I am assuming that the mere fact that "Smith is in pain" is categorical and about the present is not what makes you hesitate to allow that it is really about the future. After all we are no longer startled by the statement that "Timbuctoo is now in flames" means to a man 200 miles away with a speedometer at 200 m.p.h. that if his speedometer keeps steady while his clock revolves once he will find warm ashes when he steps from his plane. We are not startled by the doctrine that what lies beyond the horizon is a special sort of future, one that comes only if we employ not only a clock but also a car or a camel.

*Gray.* Well, I don't know. Surely "There is now a fire beyond the horizon" doesn't *mean* "There will be ashes in an hour by train".

<sup>1</sup> Quoted by N. Malcolm, *MIND*, January, 1942, p. 20.

*Brown and Black.* For heaven's sake don't let's drag in the puzzle about things beyond the horizon.

*White.* The puzzle about how we can ever know what lies beyond the horizon throws a flood of light upon the puzzle about what lies in the spiritual world behind the veil of matter. For example: Does "There's a bonfire in Trafalgar Square" mean the same to a man on the spot as it does to one in the country? And yet they speak the same language. However, forget it if you like and remember only those many statements we have already noticed which appeared to be purely categorical and about the present and yet really involved claims about the future and consider whether we have not been right in saying that the very reasons which have made us say this of them are present to make us say this of statements about the sensations of another. Relatively to "There will be poppies when summer comes" the statement "Poppies" is about the present. But when we remember how "Poppies" involves the claim that men who eat these flowers will sleep then we say "Poppies" really involves a prediction. If someone were to say "Poppies" without meaning anything about their soporific powers then this reason for saying that "Poppies" is a prediction would have gone. But others would remain. For if winds don't stir them nor mirrors reflect them they would be queer poppies. If someone were to say "Poppies" and mean nothing about their physical effects or responses but only something about their appearance, their sensory qualities, then still more reasons for his saying that he was making a prediction would have gone. But others would remain. For even if he meant to say only something about what could be seen, this would involve the claim that anyone brought to the spot would see as it were poppies. Even if we suppose that several people are on the spot and all exclaim "Poppies", this still involves the claims that if others were brought they, too, would see as it were poppies. Of course the speakers may be gods and count no one but themselves, and then, when on seeing the poppies they cry "Poppies", nothing but what is already so is involved in their statement. And on this account we may say if we like that at last we have a statement which really does not involve a prediction. We may notice that at the same moment we reach a statement which can't be wrong. We may notice, too, that this is because at the same moment their joint statement merely describes their joint sensation. But if we say that such a statement is not a prediction and cannot be wrong we must remember that just what made it true that while they counted others their

statement was predictive, also makes it true that for any subgroup of them the statement is predictive until that subgroup decides not to count the others, and thus the statement becomes descriptive merely of its sensations. And this holds when the subgroup has only one member. "Poppies" is for him in so far as it is not merely a description of his own sensations of the moment, a prediction. This is so whether his sensation is as of poppies or as of poppies with as it were voices crying 'Poppies'. For just as before it involved the claim that not merely did it seem to him that mirrors were reflecting poppies but that there were real mirrors really reflecting them so does it now involve the claim that not merely does it seem to him that people are seeing poppies but that there are real people really seeing them. Its involving the claim that mirrors and cameras could "see" poppies was a complex matter made up of such things as the speaker's counting what he found when he stretched out his hand and felt no real mirror or looked and found no photograph. Likewise whether a man takes "Poppies" to claim that other people will see poppies is a complex matter made up of such things as his counting what he finds when he stretches out his hand and feels no real people or asks, "what do you see?" and gets no answer or the answer "Nothing". Of course a speaker may neglect all this but just that transforms his statement into one merely about his own sensations. While it is about the sensations of others he counts all that. And while he does the statement involves a claim about the future. That is while it is a statement about what another sees it involves a claim about the future.

Therefore this as well as every other statement of fact, except one descriptive merely of one's own sensations of the moment, involves a claim about the future and therefore by our sixth discussion cannot be known.

If we now push on from Solipsism to Scepticism and see how with knowledge of the sensations of the moment we have no more reached perfect knowledge than we have made a perfect man when we have made it inconceivable that he should err, if now we see why this knowledge isn't knowledge, or, if you like, what sort of knowledge it is, then we shall see how it is as absurd to speak of knowing the feelings of another in the way one knows one's own as it is to speak of knowing the future in the way one knows the present, or the distant in the way one knows the near. If we knew the distant as we know the near it would be near. If we knew the future as we know the present it would be present. If you knew how things now seem to me

in the way I know how they seem to me that would be their seeming so to you. If you knew my present sensations as you know your own they would be yours. This is not grasped only when one has not grasped what sort of knowledge one has of one's present sensations and confuses it with the knowledge one may or may not have of the sheep or teaspoons one has at the moment.

"He is in pain and knows it", and "He is in pain but doesn't know it" are queer expressions. So is "He fancies he's in pain". And "He is in pain but doesn't feel it" is a contradiction, unless a use is carefully provided.

"He is in pain but doesn't know it" is not only unlike "He is in bed but doesn't know it" but also unlike "He is in love but doesn't know it". And "He thinks he's in pain but he isn't" needs careful explanation as compared with "He thinks he's in bed but he isn't" and even "He thinks he's in love but he isn't". And the consequence is that "He's in pain and does know it" is very queer too. What would it be to be in pain and not to know it? Well of course "I am in pain" isn't so sharply separated from "I am in love" as this question suggests. Nevertheless we may imagine it more sharply separated so that a speaker uses "I am in pain" or "I have a pain in my foot", so that subsequent sensations, such as those of finding he has no foot or the sudden disappearance of the pain, are quite irrelevant. This is the use we are concerned with; for otherwise we have not got what the Solipsist would call a pure description of the sensation of the moment. The purer, the less predictive the description the queerer the question, "What would it be like to feel pain and not know it?" or again, "What would it be to think oneself in pain and be wrong?"

What is it to have a sensation and not know one is having it? Although this expression has not an established use it is easy to describe a use which will tempt everybody. We say: "When I came into this room I heard a buzzing but I didn't then know I was hearing it. *Now* I do." When we ask ourselves, "And what's the difference?" we are apt to answer, "*Now* I am *saying* I am in pain". I am not merely uttering the words, I also understand and accept them. I can understand the words "I am in pain" when I am not in pain just as I can understand the words "I am in bed" when I am not in bed. So if knowledge that I am in pain is infallible it is not infallible like necessarily true statements, which are infallible because one can't understand the words of them without their being true.



To know directly that one is in pain is to say that one is and to say it on the basis of being in pain. To know that one hears a buzzing is, on the basis of hearing a buzzing, to say that one hears a buzzing. This account of knowing and not knowing that one has a sensation fits very well with the few things we do say about this. People have said that animals feel pain but don't know it. Well, of course animals never say anything. And then a person may say to an analyst, "I felt this ache in my chest yesterday when I mentioned . . . but I didn't realise it". And once the patient really says he has a certain feeling it is absurd to say that he has it but doesn't know he has it.

But if we use 'know that I have the sensation S' in this way we must realise that it is a queer kind of knowledge. Knowledge that jazz isn't music or that 1 plus 1 makes 2 is knowledge; but it's a queer kind of knowledge. To know that something is so, is with a proper basis to believe that it is so and be right. That is not be wrong. And to know, in the direct way we are thinking of, that one is in pain is to believe it on the basis of feeling pain. But what would it be for this belief on such a basis to be wrong? There is no answer. And thus no point in adding "and the statement is right", no answer to "What would it be for such a statement made under such circumstances to be right, not wrong?" We may say if we like that when a man says that S seems now to him to be P on the basis of its seeming now to him to be P then this isn't knowledge because there is no account of what it would be for him to be wrong and thus no account of what it is for him to be right—that as he can't be wrong he can't be right. Or we may say: As he can't be wrong he must be right and this is perfect knowledge. The last is a confusing way of speaking; for we use the expression, "must be right", in quite other connexions—namely where we find that a man has compelling though not demonstrative evidence that he is right. And while we talk in the same way of a man's knowledge that he is in pain we feel that this is the same sort of thing only more so. But in the cases in which the evidence is not demonstrative the whole point of adding "he can't be wrong", "he must be right", depends upon the non-demonstrativeness of the evidence, *i.e.* on the fact that he can be wrong. So if we say that a man who on the basis of feeling pain is saying he is in pain, that he really knows he is in pain then we must remember that we are simply saying that he is in pain and that he is saying that he is. That is, we must realise how the nature of the possible rightness of the belief, of the statement made, and thus the nature



of the knowledge, depends upon the nature of the possible wrongness and ignorance.

To put things another way: The peculiarity of statements of the sort, "X is in pain", "X sees as it were poppies", in short, of sensation-statements, does not lie in the fact that they can't be wrong. Even if it were a fact it wouldn't be peculiar to them for "1 plus 1 makes 2" can't be wrong. Besides they can be wrong. They can be wrong even when their subject makes them; for X may lie. True, if X, having the sensation in question, says that he has it then he can't be wrong. Nor can anyone else who says the same, *i.e.* says that X has the sensation in question. This is not peculiar to sensation-statements. If a man who is in London says he is in London he can't be wrong. Nor can anyone else who says the same. And the peculiarity of sensation-statements does not lie in the fact that one who knows the basis of such a statement knows it's right. For if on the basis that Y is the child of a child of a child of his father's parents X states "I have a second cousin Y" then he knows he is right. No, the peculiarity of sensation-statements lies in the fact that when they are correct and made by X then X knows they are correct. To put it shortly: The peculiarity of a statement of the form *X has S* (a sensation) lies not in the fact that given its basis it is correct nor in the fact that if X is given its basis he knows it is correct but in the fact that given its basis X knows it is correct.

To put the thing yet another way: As with every sort of statement, the peculiarity of a sensation-statement shows in the peculiarity of its basis. And this peculiarity is not safely expressed by saying that its basis is the fact it states. For this is true in a sense of a statement made on demonstrative grounds. The peculiarity of, *e.g.*, I am in pain lies in the fact that one can *not* say that its basis is knowledge of the fact it states while *yet* its basis guarantees that it is correct. One can say that the basis of a statement made on demonstrative grounds is knowledge of its basis or premiss but here to have the basis is to *know* the premiss and so to have the basis is in a sense to know the conclusion. The charm of X's knowledge of his sensations lies in the fact that although having the basis guarantees that what is stated is so, and thus guarantees the knowledge, it yet is not identical with the knowledge. This happens because here the basis isn't knowledge at all—it is a sensation, or if you like the having of a sensation. But of course the basis guarantees the knowledge of what is expressed in the statement although the basis is not the knowledge, *only* because the knowledge is

no more than the basis plus the statement. This is another way of saying that to know one is in pain is to say on the basis of pain that one is in pain.

In consequence of all this if someone says to me, "At last I have found something I really know—not merely something I would know if I knew something else and not a mere tautology. At last I really know a real truth. For I know I feel a doubt-ache, I know I feel pain, I know there is a smell of poppies—at least to me there is"—then I reply, "Quite so. But why do you say 'at last' as if you had gained what you first tried to reach by imagining a man to have more and more basis for saying 'Poppies' and then tried to reach by making him claim less and less with 'Poppies'. You say: When it looks to me as if a man is eating poppies and I claim that he will fall asleep I know I am right, or at least if I claim merely that here's a man eating poppies I know I am right, or at least if I claim merely that here's something which looks like a man eating poppies I know I am right, or at least if I claim merely that here's something which looks to me like a man eating poppies I know I am right."

In all the cases except the last the connexion between the conditions stated and the consequence, "I know I'm right", is dubious. This is why each is followed by "At least . . .", and then a statement in which the connexion is less dubious. In the last the connexion is not at all dubious. And it is easy to see why. The reason is the usual one, it is that the consequent now adds nothing. And it is easy to say why it adds nothing. "X knows that S is P" if (1) he says that S is P, (2) he has a basis for what he says, and (3) he is right, *i.e.* S is P. Now in all the if . . . then statements above the antecedent provides for conditions (1) and (2), and therefore the consequent adds something only while "I am right" adds something, and this adds something only while "I am not wrong" adds something. But "I am not wrong" adds nothing when it has been said that I say "S seems to me P" on the basis that S seems to me P. For what would it be to be wrong under these circumstances? If on the basis of a sensation of pain I say I have a sensation of pain then what would it be for me to be wrong?

*Brown.* But surely a man may misdescribe his own sensations?

*White.* There is a sort of way in which a man may be wrong about his sensations and this yields a sort of way in which he may be right. Indeed our confusion about the knowledge which a man has of his sensations of the moment is due not only to our losing hold of the connexion between the possibility of

being right and the possibility of being wrong, and not only to the fact that statements about his immediate experience are not sharply separated from other statements about his states of mind at the moment, which statements can be wrong, but also to the fact that even statements about his immediate experiences can have a sort of wrongness; and so a sort of rightness.

True, when "I feel a pain in my foot" or "I see a rainbow" have become merely reports of sensation they have become exclamations in all but the indicative shape of the sentence used. But an exclamation can be inappropriate, and so be wrong and if you like false. So if someone says "Surely a man may misdescribe the sensation he is having?" the answer is "Certainly—but notice, don't forget, that misdescribing has become no more than misnaming, and not misnaming in the way in which I misname Jack when I call him Alfred, thinking him to be Alfred, but in the way in which knowing him to be Jack I call him Alfred, thinking his name is Alfred *or not caring a damn what his name is*.

Because in such a case I can be only verbally wrong it is tempting to say that I am making a verbal statement. And in so far as I am making a statement at all by calling him 'Alfred' or saying 'Ow' I am making a verbal statement to the effect that this sound is appropriate. But the fact is I am not saying of a sound which in fact is inappropriate (or when I am "right" appropriate) that it is appropriate; I am using a word which in fact is inappropriate (or when I am "right" appropriate) inappropriately (or when I am "right" appropriately). What fixes whether I speak appropriately or not is the same as what fixes whether the statement that the word I use is appropriate, is true or false. The facts which make the verbal statement false and unacceptable make the exclamation "Alfred" or "Ow" or "I am in pain" inappropriate and misleading to others though not to the speaker. Or if you like they make the exclamations "Alfred!" and "Ow!" inappropriate and the "statement" "I am in pain" "false". But if you speak like this it will be very necessary to remember that for the speaker "I am in pain" cannot be wrong in two ways like "Here is Alfred" said of a figure in the mist, which may be wrong because Alfred isn't the right name *or* because the figure isn't the man the speaker thinks it is.

If we wish to use 'wrong' so that a man is wrong if he says "I see as it were a rabbit" when he ought to have said "hare" or when he says "Ow!" when he ought to have said "Psha!" then if we want a case where he is making a statement which

he can *know* to be right we must take the case in which he is not claiming to speak appropriately, *i.e.* doesn't count anything he subsequently discovers as to how people use the words he has used.

*Gray.* Have we such a case when people play the game of giving "appropriate" names to passers-by, or when one says, "The Union Jack is really a sort of Stars and Stripes", or when Mr. Nash calls his picture which the crude might describe as book-cases in the clouds by the more penetrating name, "The Mansions of the Dead"?

*White.* No—even these don't do. For such "statements" may be winners. And by the same fact may be losers. For if people don't "see it at all" even when one says "You see here, from the centre, spreads the star, here, round the outside, are the stripes", or Mr. Nash says "You don't feel these cold and hopeless but untroubled spaces?" then the speaker loses. Of course Mr. Nash may reply, "I never expected anyone else to feel the same as I", and then he can't be wrong because they don't. He may even not expect that he himself will want to give the same name to-morrow. Even so, if, after he has said "The Mansions of the Dead" in front of his picture, we ask him, "When you spoke you felt that that was the appropriate title, didn't you?" and he replies, "No, not at all", then we shall protest saying, "But you spoke as if you felt that title appropriate". If we wish to avoid even this sort of wrongness then he must not when he speaks be taken to be claiming appropriateness for his air of finding an appropriate name nor even to be claiming appropriateness for his air of naming. Appropriateness and inappropriateness, whether of the words in the sentence or of the "sentential" air, whether relative to the practice of others or the speaker's own practice, mustn't be claimed. Only then will it be true that the utterance's context in time can never be bad for it. The "speaker's" feelings make him utter a certain sound—like an infant's first whimper. The infant can't be wrong. But then also he is not knowing anything. For to know implies being right and being right is not merely not being wrong. It involves making a claim and not being wrong. If we choose to say that an infant when it's whimpering knows it's in pain then we must remember that to say that it knows it's in pain is to say that it's in pain and whimpering.

Of course there is a difference between the new-born infant and the philosopher who, having searched for something to say which couldn't go wrong, fixes his attention on a patch of colour

or on a feeling in his head and says "Wuf—there I can't be wrong over that". About the philosopher there lingers the air of using language, especially if he says "This is wuf", though of course he might just as well say "Wis is thuf". We may say if we like just because of the pattern of sound and the serious air, that he is making a statement.

But it isn't a statement. What is now more than ever apparent is that if, and in so far as, a statement is a statement then and that far it's a prediction. Which is only to be expected. A banknote means nothing to a baby. But it does to me. I cling to it anticipating champagne or seats at the cinema. It's the same with a cheque, a ticket and a statement. And *what* they mean to me is a matter of *what* they lead me to anticipate—much or little, this or that, Bethnal Green or Central Park.

*Black.* People sometimes say, "I know it means nothing—but say it—it means a lot to me". And florists advise us to say it with flowers. And flowers aren't promises.

*White.* Nor are they statements—unless they are roses in Spain, when again they are promises. As to other flowers. Though they aren't statements they wouldn't mean so much while meaning nothing if they weren't wrong in certain contexts and right in others. But no context can make the philosopher's statement wrong. So far from being a statement it's not even an exclamation, not even an exclamation in his own language.<sup>1</sup> It is only an "exclamation" in his own "language" of the moment. Or if you like it is an *absolute* exclamation. The name is a good one because it suggests that it is that sort of utterance which remains when we have removed more and more and finally all those features of exclamations which permit us to say that they are still a sort of statement. These features had to go because they are the very ones which compel us to allow that a speaker who makes one can be wrong in a sort of way and so doesn't altogether know that he is right. It is better not to call them statements, for then, intoxicated with the fact that made under the prescribed circumstances they can't be wrong, we are apt to forget that though we have gained on the roundabouts what we've lost on the swings we have also lost on the swings what we've gained on the roundabouts.

If we insist on calling such extremely out of the way utterances statements and descriptions we needn't be surprised to find that they logically can't be wrong, and we must remember that just this is what we mean by saying that they are logically

<sup>1</sup> People who have heard Wittgenstein will know how much this about such a sentence saying nothing comes from him.

bound to be right. If we call a "walk over" a race we needn't be surprised that there are races on which one can't lose. But it will then be well to remember that this is all we mean when we say there are races on which one is bound to win. I prefer to say that a "walk over" is not a race or that there are races on which one can neither win nor lose.

It is inappropriate to be surprised by the fact that there are pairs of numbers such that when the first is added to the second the sum is no greater than the first, once we have said that 0 is a number.

It is the same with statements of the form *If S then P*, such as "All men are mortal" or "With a movable pulley W will balance  $\frac{1}{2}W$ " or "No statement can be really known to be true"—they become more really knowable, more necessarily true, the fewer future facts can make them fail and they become quite knowable and quite necessarily true only when no future fact can make them fail. This is so when what makes the speaker call a thing S makes him call it P, that is, they may be reached by increasing what is given in S or reducing what is claimed in P until these are equal. And then the statement is a tautology. At the moment a statement becomes a necessary one, *i.e.* such that wrong or right it can't be wrong in the way that a contingent statement is, that moment it can't be right in the way that a contingent statement is. Of course if we call it wrong when the speaker's use of 'S' and 'P' is eccentric then we can call it right when it isn't. There is here again an inclination to identify a statement in which words are used as others would or as they wouldn't with the verbal statement that others would so use those words. The cause for this is again a grammatical muddle. What makes the verbal statement false makes the necessary statement eccentric, *i.e.* wrong in the way in which it can be wrong. Likewise for right.

Another point from the categorical series is reflected here. If we wish to avoid calling one who makes a necessary statement wrong just because he is eccentric, we may say that he is right in his own language or we may choose a speaker who is claiming to be right only in his own language. Such an utterance we may call a *private* tautology. If we wish to avoid his being wrong on account of eccentricity relative to his own language we shall have to say, "He was right in the 'language' he was speaking at the moment he was speaking", or we must choose a speaker who is "claiming to be right" only in the "language" he is speaking at the moment he is speaking, *i.e.* disregarding not only others but himself at other times. This is an *absolute*

tautology. And about such a statement the speaker knows indeed that there can be no mistake. Here again is perfect knowledge, and here again the absolute exclamation. The route is different but the journey's end the same.

Conditional or categorical, critical or factual, necessary or contingent, public or private, about others or about oneself, about what is or about what seems, the same principle holds : No stakes, no winnings. Like the man with the rainbow-moment, we reach at last a place where Time and Chance can do no more. But alas too late. For the moment that security is reached that moment it is worthless.

*(To be continued.)*

## II.—HISTORICAL EXPLANATION.

BY MORTON G. WHITE.

HISTORIANS and philosophers frequently speak of what they call historical explanations. Some explanations, they say, are physical, others chemical, and still others historical. In this paper I shall discuss several questions which arise in connection with the concept *historical explanation*. First I shall consider the view, which is widely held, that a historical explanation is one that involves a reference to the past in a way that distinguishes it from all other kinds of explanations. But more concretely, the proposed analysis of the phrase "historical explanation" which I shall consider is the one according to which a historical explanation explains facts prevailing at one time by reference to facts prevailing at an earlier time.<sup>1</sup> I shall try to state this view as clearly as I can, and then try to show that it is enticing, but incorrect, because it prevents us from distinguishing many explanations which we call non-historical from historical explanations. In the remainder of the paper I shall try to show how the problem of analysing historical explanation is connected with the general problem of analysing what it means to say that an explanation is of a certain kind. The paper does not present a definition of "historical explanation", but in its attempt to clarify some problems preliminary to the presentation of such a definition, it may be regarded as an introduction to a logical analysis of history.

### I.

When one distinguishes a physical explanation from a biological explanation, or from a chemical explanation, one has a fixed meaning of the word "explanation" in mind. The physical, chemical, and biological explanations are all regarded as explanations in the same sense of that word. When one goes on to compare historical explanations with these other explanations, the presumption is that the word "explanation" has not shifted in meaning. All these explanations differ only in being different kinds of explanations. I stress what may seem to some very obvious only because I do think that it is *possible* that the word

<sup>1</sup> Thus, Prof. Hugh Miller in his *History and Science* (Berkeley, 1939) says: "a genetic science is one which understands the present in the light of the past" (p. 21).



"explanation" when it appears in the phrase "historical explanation" means something different from what it means when it appears, say, in the phrase "physical explanation". But, although I admit the logical possibility of the word being so ambiguously used, I admit no more than a *logical* possibility. For I think that an examination of the usage of historians and philosophers shows that when they speak of historical explanations, they do not use some other sense of the word "explanation". What other meaning might be reasonably attached to the word by an analyst of usage, I do not know, but I think it just as well to record this possibility, if only to make explicit one of my own assumptions.

For purposes of the present paper I shall accept a brief analysis of scientific explanation recently presented by C. G. Hempel. Hempel says :

"The explanation of the occurrence of an event of some specific kind  $E$  at a certain place and time consists, as it is usually expressed, in indicating the causes or determining factors of  $E$ . Now the assertion that a set of events—say, of the kinds  $C_1, C_2 \dots C_n$ —have caused the event to be explained, amounts to the statement that, according to certain general laws, a set of events of the kinds mentioned is regularly accompanied by an event of the kind  $E$ . Thus, the scientific explanation of the event in question consists of (1) a set of statements asserting the occurrence of certain events  $C_1, \dots C_n$  at certain times and places, (2) a set of universal hypotheses, such that (a) the statements of both groups are reasonably well confirmed by empirical evidence, (b) from the two groups of statements the sentence asserting the occurrence of event  $E$  can be logically deduced."<sup>1</sup>

As an illustration, consider the following. Someone discovers that the part of Fifth Avenue, New York City, lying between 42nd and 43rd Streets (" $f$ " for short) is wet at 4 P.M., April 1, 1942. He asks why, and is given the answer: "Because  $f$  was rained on at 3.59 P.M., April 1, 1942." The universal hypothesis which, in conjunction with the second singular statement, permits the deduction of the statement designating the explained fact, is: If any object is rained on at time  $t$ , then it will be wet one minute after  $t$ . Put schematically, using " $W$ " as the name of the relation expressed by the formula " $x$  is wet at time  $t$ ", and " $R$ " as the name of the relation expressed by the formula " $x$  is rained on at time  $t$ ", we have the following deductive argument.

1.  $f R$  3.59 P.M., April 1, 1942.
2. For every  $x$  and for every  $t$ , if  $x R t$ , then  $x W$  one minute after  $t$ .

<sup>1</sup> C. G. Hempel, "The Function of General Laws in History", *Journal of Philosophy*, vol. 39, p. 36 (1942).

Therefore,

3. *f* W 4 P.M., April 1, 1942.

The structure of this explanation is fairly clear. The first statement asserts that a certain material object has a certain relation to a certain date. The second asserts that if an object has a certain relation to a certain date, then that object will have another relation to another date. Not all explanations fall into this pattern; but this is a very common sub-class of the class of explanations. I choose it both because of its simplicity and because it is an explanation in which a fact prevailing at one time is explained in terms of a fact prevailing at an earlier time. So far as I am able to discover, this explanation furnishes us with the pattern for what some people call historical explanations. Let us compare it with an explanation which would not be called historical on this view.

Consider the kind of explanation in which the fact explained is designated by a statement of the form  $x \in P$ , i.e. a statement asserting that an object is a member of a class. For want of a more interesting example at the moment, consider the statement " $a \in \text{mortal}$ ", where " $a$ " names some entity. Suppose someone should ask: "Why is  $a$  mortal?" and that someone else should reply: "Because  $a$  is a man". The scheme of this explanation is:

1.  $a \in \text{man}$ .

2. For every  $x$ , if  $x \in \text{man}$ , then  $x \in \text{mortal}$ .

Therefore,

3.  $a \in \text{mortal}$ .

The two explanations we have considered differ in at least one very important respect. The first can be described as an explanation in which a fact prevailing at one time is explained by reference to a fact prevailing at an earlier time. The fact that  $f$  is rained on at 3.59 may be said to be earlier than the fact that it is wet at 4. The second explanation cannot be described in this way. The fact that  $a$  is a man is no earlier than the fact that  $a$  is mortal. The main difference between both explanations depends upon the difference between the generalisations that guide them. In the case of the illustration about Fifth Avenue, a generalisation is involved which refers to a date  $t$  in its antecedent, whereas the consequent contains a reference to a date later than  $t$  by some fixed amount of time. This is not true of the second illustration.

It must not be inferred that the difference between the first and second illustrations depends solely on the fact that the singular statements in the first express relations between objects

and dates, or on the fact that the generalisation which guides the first contains a time-variable explicitly.<sup>1</sup> For the following case provides a good counter-example. Let us assume that the statement, "Whenever Chinamen eat, they eat with chopsticks" is true. Now suppose I ask why Chiang is eating with chopsticks at 7 P.M., May 1, 1942, and this is explained as follows:

1. Chiang  $\in$  Chinaman and Chiang is eating at 7.
2. For every  $x$  and for every  $t$ , if  $x \in$  Chinaman and  $x$  is eating at  $t$ , then  $x$  is eating with chopsticks at  $t$ .

Therefore,

3. Chiang is eating with chopsticks at 7.

This is an explanation in which the singular statements take the form  $x R t$ , and yet one which would not be called an explanation which explains a fact prevailing at one time by reference to an earlier fact. Clearly, the fact that Chiang is a Chinaman who is eating at 7 is no earlier than the fact that he eats with chopsticks at 7. The point to be stressed is that the generalisation immediately above is not one which refers to *different* dates in its antecedent and consequent.<sup>2</sup>

Having interpreted the view as I have, I shall now state my objection to it. The point is that there are explanations which would be called physical, others which would be called chemical, and others which would be called biological, all of which must be called historical, on the view. For instance, if one explains the present relative positions of the sun, the moon, and the earth, by reference to their relative positions one year ago, one is giving a *historical* explanation on the view under consideration.

<sup>1</sup> E. Zilsel, in his paper "Physics and the Problem of Historico-sociological Laws", *Philosophy of Science*, vol. 8, pp. 573 ff. (1941), distinguishes between what he calls temporal laws and simultaneity-laws by saying that the former contain time-variables explicitly, whereas the latter do not. My point is that there are laws which contain the time-variable explicitly, and which would not be called "temporal", i.e. would not be said to connect facts prevailing at different times. They could be what Zilsel calls "simultaneity laws" and still contain the time-variable explicitly.

<sup>2</sup> I have, in the above, formulated as clearly as I can, the view that a historical explanation is one that explains the present in terms of the past. One of the conditions which guided my interpretation is that the holders of this view want to distinguish historical explanations from other kinds. In the light of this they could not have in mind the sense in which *all* explanations explain the present in terms of the past. I have in mind the fact that every explanation presupposes a generalisation, and that every generalisation is dependent for its verification upon some facts which are past. If they had meant this by the phrase "explain the present in terms of the past, or by reference to the past", they would have to call *all* explanations historical, and surely this is not their intention.

Obviously the statement of their positions one year ago expresses a fact earlier than the one which expresses their present positions. Furthermore, the laws of mechanics, which figure in the explanation, connect facts prevailing at different times. But we do not want our analysis to result in the statement that one explanation is both mechanical and historical. One of the conditions we impose on our analysis is that it permit us to deny this possibility. Put in other words, we are assuming that the phrase "historical explanation" is so used that we cannot say of an explanation, without impropriety, that it is both physical and historical. It is not my intention to deny that one could use the phrase "historical explanation" so that historical explanations (in this arbitrary sense) would turn up in all sciences. No doubt the kind of explanation that explains present facts by referring to past facts (in the above sense) is an interesting kind, and one that does appear in all the sciences. But if we are interested in analysing what is actually meant by the phrase "historical explanation", we should do well to assign another name to the class of explanations that explain present facts in terms of past facts.

Although we have rejected one analysis of the phrase "historical explanation", we have not thereby destroyed the possibility of its having another meaning. Indeed, we have implied that it does. But since we have tried to show that explaining present facts by means of past facts does not distinguish historical explanations from others, we must have some general idea of the criteria which serve to distinguish different kinds of explanations from each other. The next part of the paper, therefore, will be devoted to an analysis of what it means to say that one explanation is different in kind from another.

## II.

An explanation, we have seen, is a constellation of statements. We have also seen that some explanations are said to be different in kind from others; some are said to be physical, others chemical, and others historical. If we believe that the terms "physical", and "chemical" are on a par with the term "historical" when all these are applied to explanations, then the general method we use in distinguishing a chemical explanation from a physical explanation is the one we use in order to distinguish a historical explanation from other kinds of explanations. The problem, if it is conceived in this way, involves a consideration of the relation between an explanation and the

discipline in which it is an explanation. When we say that a given explanation  $e$  is a physical explanation, and that another,  $f$ , is a chemical explanation, both of these statements may be regarded as instances of the statement form:  $x$  is an  $S$ -ical explanation, where for " $x$ " we put names of particular explanations, and for " $S$ " initial fragments of the names of sciences, e.g. "chem", "biolog", "histor", etc.

It will be recalled that one of the conditions we thought an analysis of "historical explanation" should satisfy was that no explanation could be both a historical explanation and some other kind of explanation. This is true, not only for historical explanations, but for all kinds of explanations. The condition may now be stated quite generally. It amounts to saying that the formula " $x$  is an  $S$ -ical explanation" expresses a functional, or many-one relation between  $x$  and  $S$ .

Normally a biologist who has discovered a certain singular biological fact looks to the laws of biology and to other biological facts for his explanation. In such a case the statement expressing the fact to be explained is a biological statement, so is the generalisation that guides the explanation, and so is the statement expressing the so-called initial condition. Here, we would say that the biologist is presenting a biological explanation, the reason being that every statement in the explanation is a biological truth. We define a biological explanation, therefore, as an explanation everyone of whose constituent statements is a biological truth. And the definition may be generalised, so that to say that  $x$  is an  $S$ -ical explanation is to say that every statement of  $x$  is an  $S$ -ical truth. If we push the analysis further we have to ask what it means to say that a given statement is an  $S$ -ical truth.

J. H. Woodger has presented a rigorous formalisation of a part of biology which permits us to discuss this question in a concrete way.<sup>1</sup> He presents, in his monograph, *The Technique of Theory Construction*, a sample biological theory which he calls  $T$ .  $T$ , of course, does not include all of biology; it is only a part of biology. From a methodological point of view, however, it serves our purpose, since it serves to elicit the points we have in mind. At first, the consideration of this theory may seem colossally irrelevant to the problem of historical explanation. Later, it is to be hoped, its relevance will be obvious.

The first point of significance is the distinction Woodger makes

<sup>1</sup> J. H. Woodger, *The Axiomatic Method in Biology* (Cambridge, 1937) also a monograph, *The Technique of Theory Construction*, International Encyclopedia of Unified Science, Vol. 2, no. 5 (Chicago, 1939).

between two kinds of constants.<sup>1</sup> Constants are either (a) logical constants or (b) subject-matter or descriptive constants. The logical constants—those like “not”, “and”, “or”, “some”, and “all”—are those which appear in every scientific theory, and not only in the specifically biological theory *T*. The subject-matter constants of the theory are “*P*”, “*T*”, and “cell”. “*P*” denotes the relation *part of*, “*T*” the relation *before in time*, and “cell” denotes the class of biological cells. All the signs other than these three which appear in the theory *T* are either logical constants, variables, or subject-matter constants which can be defined with the help of the three primitives, logical constants, and variables.

After specifying the various ways in which statements may be formed in this theory, Woodger goes on to set up a list of statements which he calls Part I of the theory *T*. These are all logical truths—statements from the calculus of propositions, the calculus of classes, the calculus of binary relations, etc.—which figure in the proofs to be made later on. It turns out, then, that Woodger’s biological theory *T* contains one part comprising only logical truths. Logic, it is then said, is a discipline which precedes the specifically biological part of the theory. To say that logic precedes or that it is presupposed means that “all expressions and laws of logic are treated on an equal footing with the primitive terms and axioms of the discipline under construction; the logical terms are used in the formulation of the axioms, theorems and definitions, for instance, without an explanation of their meaning, and the logical laws are applied in proofs without first establishing their validity.”<sup>2</sup> Thus, we see that in so far as some logical truths appear in Woodger’s theory *T* there are some truths in it which are not *specific* to *T*.

The important point is that every theory can have its statements divided into two classes—those which are statements of the disciplines presupposed by the theory, and those which are specific to the theory. In the case of Woodger’s theory, Part I is said to contain all and only those statements coming from theories presupposed by *T*, whereas Part II is said to contain all and only those which are specific to *T*. If we regard all statements, regardless of whether they fall into Part I or Part II as statements of *T*—or, in our idiom, as *T*-ical statements—we construe the notion of *T*-ical statement in a very wide sense.

<sup>1</sup> This distinction is drawn in Alfred Tarski’s *Introduction to Logic* (p. 18). Woodger’s discussion involves an application of this point to a concrete, simple example.

<sup>2</sup> Tarski, *op. cit.*, p. 119.

Not only those statements which we normally regard as statements of the science are said to be statements of the science, but also those which come from sciences presupposed by the science. In order to avoid ambiguity, we shall, hereafter, distinguish carefully between the phrase "*S*-ical" and the phrase "specifically *S*-ical", for any science *S*.

If we return now to the problem that turned our attention to Woodger's theory, we must revise our definition of "*S*-ical explanation". We said, it will be recalled, that an *S*-ical explanation is an explanation every one of whose constituent statements is an *S*-ical truth. We should revise that to read: an *S*-ical explanation is an explanation every one of whose constituent statements is a *specifically S*-ical truth.<sup>1</sup> What, now, is a specifically *S*-ical truth?

A specifically *S*-ical truth is one that contains some specifically *S*-ical terms in an essential way.<sup>2</sup> On Woodger's analysis, the statement that the relation *P* is transitive seems to be regarded as a specifically *T*-ical truth.<sup>3</sup> It is considered a specifically *T*-ical truth because it contains at least one subject-matter constant of *T* in an essential way. I have included the condition that the statement contain subject-matter constants in an *essential* way in order to prevent statements like "Whatever is a cell is a cell" from being called specifically *T*-ical. In this statement the constant "cell" appears, but it does not appear essentially, *i.e.* any other word which could, with grammatical correctness, be put in place of "cell" would leave the statement thus formed true. On the other hand, this is not so in the case of the statement "*P* is transitive", since there are some replacements for "*P*" which would yield a falsehood. Thus, there are two conditions that a specifically *T*-ical truth must satisfy:

<sup>1</sup> It should be noted that Hempel's definition of "explanation", which we have adopted, excludes logical truths from the class of statements constituting an explanation. If it did not, our class of explanations, *i.e.* the class of *S*-ical explanations, where "*S*" took as substituends the initial fragments of names of empirical sciences, would be empty. This would be the case because some logical truths would always figure as premises of deductions, and their presence would assure the presence of some truths not specifically *S*-ical.

<sup>2</sup> It is very important to remember that "*S*" can take as substituends only the initial fragments of the names of *empirical* sciences. The definition would fail in the case of specifically logical truths. It is not true to say that a specifically logical truth is the same as a truth that contains some specifically logical terms in an essential way. All statements contain some specifically logical terms in an essential way.

<sup>3</sup> Below we shall consider the question whether Woodger would be right, from our point of view, in saying that this statement is specific to *T*.



(1) it must contain specifically *T*-ical terms, and (2) these terms must occur essentially in some places.<sup>1</sup>

Two things should be observed in connection with the definition of "*S*-ical explanation" that we have presented. First of all, if we define an *S*-ical explanation as one which is composed solely of specifically *S*-ical truths, it follows that the formula "*x* is an *S*-ical explanation" expresses a many-one relation, because, for any explanation *x* we choose, there is no more than one *S*, such that *x* is an *S*-ical explanation. This, we remember, is one of the conditions we imposed on our analysis. The second point to be observed is that whether an explanation is *S*-ical depends ultimately, on whether specifically *S*-ical terms appear in it. It follows from this that the existence of *historical* explanations depends upon the existence of specifically historical terms.

We have, in what may seem to be a very roundabout way, reached one of the most serious problems of the paper. Are there any specifically historical terms? If there are, what are they?

### III.

Before going on to the direct consideration of these questions, is necessary to make a few more general observations.

Because logical constants are used in the formulation of statements in Woodger's theory *T*, and because logical laws are used in making deductions in the theory, logic is said to be a discipline which precedes or which is presupposed by this small part of biology. It should not be thought, however, that logic is the only discipline that an empirical science can presuppose, even though logic is presupposed by every empirical science. A given empirical science may presuppose, in addition to logic, other empirical sciences. This fact is even demonstrated by Woodger's theory *T*.

Although Woodger makes only a bipartite distinction between the constants which figure in his theory—between those which are logical, and those which are descriptive—it is possible to make a tripartite distinction if one thinks of the number of disciplines whose terms are involved in his small theory *T*. It will be recalled that Woodger listed three signs as his subject-matter constants, without making any distinction between them. One was "*P*", denoting the relation *spatial part of*, the second was "*T*" denoting the relation *before in time*, and the third was

<sup>1</sup> For a detailed analysis of the notion of the essential occurrence of a term, see W. V. Quine, "Truth by Convention", *Philosophical Essays for Alfred North Whitehead*.



"cell", denoting the class of cells, in the biological sense. An examination of these three constants will show that the first two are distinctly different from the last in a certain respect. The expressions "is a spatial part of", and "is before in time", appear in many scientific discussions which do not deal with biological matters. They are terms that are not specific to biology, and therefore not specific to the small part of biology which Woodger formalises in his monograph. They, like the logical terms, may be regarded as terms taken over from other disciplines. Therefore, those truths in Woodger's theory which contain either or both "*P*" and "*T*" as their only essential, non-logical constants, may also be regarded as truths which are not specific to the theory *T*. These terms come from disciplines in which they are treated independently of any biological terms like "cell".<sup>1</sup>

## IV.

After all these discussions of general methodological points, I should like to return now to the main problem of the paper—the issues involved in the analysis of the phrase "historical explanation". It will be recalled that one of our previous conclusions is that if historical is a special case of *S*-ical explanation, as we have defined "*S*-ical explanation", then a necessary condition for the existence of historical explanations is the existence of specifically historical terms.

It should be obvious from the start that the search for the terms which are specific to history provides the most difficult problem of its kind. If we proceed empirically, and examine history books in an effort to determine which terms are specific to history we find ourselves in a morass, chiefly because of the number of terms which come from other sciences. The matter is not as simple as it is in connection with Woodger's small theory. There we find it relatively easy to single out those terms which are specific to the theory, as opposed to those coming from presupposed sciences. Woodger's theory is axiomatised and therefore it presents us with the answer to the question we are interested in, to a certain extent. It provides, from our point of view, a logical analysis of the theory. If we could do for history what Woodger has done for a small part of biology, we should have answers to some of the questions raised in this paper. But one

<sup>1</sup> "The general theory of the concept 'part of' has been developed by S. Lesniewski under the name of *Meréologie*", Tarski says in the appendix he contributes to Woodger's *Axiomatic Method*, p. 161, n. 1.

would hardly be making an unguarded claim if one called this unlikely. Although there is nothing self-contradictory in the idea of an axiomatised history, I do not think that we can plausibly expect it to be fulfilled. But it does not follow from this that we are not able to state some of the terms which are specific to history. This is a much smaller order, and can be accomplished, in many cases, without setting up a rigorous axiom-system for the discipline under consideration. Surely this is the case for economics, for instance. The fact that history is neither axiomatised nor likely to be axiomatised, therefore, is not a serious difficulty.

An examination of any history book will reveal terms that would hardly be called specifically historical and yet which are necessary for the expression of what are called "historical facts". Take a simple statement of historical fact like "Constantine defeated Maxentius at the Milvan Bridge on October 28, 312", which is likely to appear in any history of the Roman Empire. Although it is highly unlikely that the term "defeated" is a physical term, and although many philosophers hold that the terms "Maxentius" and "Constantine" are not physical terms, there can be no doubt about the terms "the Milvan Bridge" and "October 28, 312". The last two are names of places and times, and these are surely not specific to history; they are physical terms which are essential to the expression of a historical fact in the way that logical constants are. But because there are terms in this statement which occur in it essentially and which are neither logical nor physical, we cannot call it a physical statement. However, there are other statements which appear in history books which are composed of only physical and logical constants; in short, there are physical statements which appear in history books. To take a simple example, a historian of the middle ages tells us that Anjou, Normandy, and Gascony were shaken by earthquakes in 1207, 1214, and 1223 respectively.<sup>1</sup> Thus history may be said to presuppose physics. The same historian tells us of the existence of famine in twelfth-century France. "Famine" is no physical term, but it is not a specifically historical term. Thus history also presupposes biology, or whatever science contains the word "famine". The relation of presupposition, as it has been defined, helps us to clarify the statement, frequently made by philosophers, that many "factors" enter into history. Without going into this in detail, we may say that part of what this means is that historical statements frequently contain terms from other sciences, and that historical

<sup>1</sup> A. Luchaire, *Social France in the time of Philip Augustus*, p. 1.

works contain empirical statements that are not historical. In short, it means that history presupposes an enormous number of sciences other than itself.<sup>1</sup>

Let us continue our comparison of the statement "Anjou was shaken by an earthquake in 1207"; and "Constantine defeated Maxentius at the Milvan Bridge on October 28, 312". We have observed that the first is historical, but not specifically historical, whereas the second is a specifically historical statement. Two points follow: First, that an explanation made up solely of statements of the first kind (*i.e.* historical, but not specifically historical statements) would not be a historical explanation. This is shown by the fact that a seismological explanation of *why* the earthquake took place in Anjou in 1207, would not be called a historical explanation. It also follows that an explanation of why Constantine defeated Maxentius, if it contained as constituents only specifically historical truths, would be a historical explanation.

These two points deserve elaboration. (1) The fact that a historian would not be impelled to explain why an earthquake took place in Anjou in 1207 indicates the role which physical statements play in history. In general, physical statements will appear only if the historian believes that they can serve as initial statements in an explanation terminating in a specifically historical statement. In other words, physical truths appear in history books, not because historians are interested in physics, but rather because they are interested in all the facts which will help them explain specifically historical facts. This is not only true of physical statements, but of all statements coming from sciences which are presupposed by history. This brings us to our comment about Constantine's defeat of Maxentius in the last paragraph.

In saying that an explanation of why Constantine defeated Maxentius would be historical, it was specified that this explanation had to contain "only specifically historical truths". The need for this condition, of course, flows directly from our definition of "historical explanation". But it also reveals an important possibility which we have only mentioned, but have not discussed. It implies the possibility that a specifically historical fact might be explained by referring, not to specifically historical facts, but to facts that are not specifically historical. In other words, it suggests a distinction between what we have called a historical explanation and an explanation of a specifically historical fact.

<sup>1</sup> In this respect history may be taken as the opposite extreme of logic, which presupposes no science.

Not all explanations of specifically historical facts are historical explanations.

Luchaire,<sup>1</sup> after discussing the various famines in twelfth-century France, says: "Famine produced brigandage". The illustration suits our point very well. No doubt the generalisation involved is not very clear, and one might hesitate before saying that all famines are followed by an amount of brigandage which would not follow if famine did not occur. Nevertheless, whatever qualifications are necessary to make the generalisation involved true, the resulting statements would contain the terms "famine" and "brigandage". It is reasonable to construe "famine" as a biological term and "brigandage" as a historical term. It would also seem that the initial statements in the explanation express a biological fact to the effect that a group of people were in a certain biological state. The statement deduced, however, would express something to the effect that a group of people were in a certain social state. The generalisation which, when conjoined with the biological statement, implies the specifically historical statement, would contain both biological and specifically historical terms. Taken all in all, the explanation would not be historical simply because of the appearance of one specifically biological statement, *i.e.* the statement of the fact that the famine occurred.

This raises a very important question. Are there, it may be asked, any explanations of the kind we have just discussed? In other words, are there any explanations of specifically historical facts which appeal only to biological facts in the manner of the above explanation. When this question is raised we are forced to examine our illustration more carefully. We must ask whether the historian really intended to say that the mere presence of a certain biological phenomenon was enough to account for the specifically historical phenomenon that followed. Is it not likely, we may ask, that the historian would have to qualify his generalisation: "Famine produced brigandage", so that it would read: "Famine, together with another condition designated by a specifically historical term, produced brigandage"? If this question is answered in the affirmative then we might be impelled to raise the same question about all explanations which are supposed to explain historical facts by reference to only non-historical facts. If it can be shown that all the generalisations involved have to be revised in the way suggested, it is obvious that the statement of an initial condition would never be anything but a specifically historical statement. The generalisation

<sup>1</sup> *Op. cit.*, p. 18.

guiding the explanation would relate the appearance of famine plus some specifically historical condition, with the specifically historical condition to be explained, and the statement of the initial condition would involve an affirmation of the antecedent of the generalisation. But the antecedent of such a generalisation would assert the presence of a specifically historical condition and another condition. The mere presence of a specifically historical term in this conjunction of statements, however, would make the conjunction a specifically historical statement. Now, the generalisation guiding the explanation also contains a specifically historical term in an essential way, and therefore it too is a specifically historical statement. The same is obviously true of the statement expressing the fact to be explained.<sup>1</sup> Since all three constituent statements are specifically historical, the explanation is a historical explanation.

The question which is raised is one whose answer I am not able to give. The problem, however, is fairly clear. We must try to find out whether there are explanations of historical facts which are not historical explanations, the latter phrase being interpreted as we have in this paper.

## V.

Thus far, although we have spoken frequently of specifically historical terms, we have not stated explicitly what we mean by the term "specifically historical term". The reader has been given some suggestions, but at no point have we defined the notion. The present section of the paper will not terminate with such a definition. In it, however, an attempt will be made to remove certain misunderstandings of the problem.

We have seen how history presupposes other sciences. It presupposes physics insofar as it refers to times and places, and assumes the truth of physical laws; it also presupposes physics

<sup>1</sup>The following question may be raised. Since the generalisation which guides an explanation of a specifically historical fact, is a specifically historical statement, and since the explanation may be regarded as composed of two statements—the statement to be deduced and a conjunction of all others—and since this conjunction, insofar as it contains the generalisation, is a specifically historical statement, why isn't every explanation of a historical fact a historical statement? Obviously the statement expressing the fact to be explained is specifically historical, and so is any conjunction containing a specifically historical statement, specifically historical. To meet this question adequately we should be forced to define the notion *constituent of an explanation* so that the conjunction of a generalisation guiding an explanation and statements of initial conditions, is not a statement which is a constituent of an explanation.

insofar as it tries to establish as many singular physical truths as will contribute to the explanation of specifically historical facts. History also presupposes sciences like chemistry and biology. In fact, it seems almost impossible to put a limit on the number of sciences history does presuppose. Thus far we have mentioned only the sciences which do not study the specifically human or purposive behaviour of human beings. But obviously history presupposes all of the sciences that deal with human purposive behaviour. In fact, the difficult thing is to say where they stop and where history begins. In other words, the terms which are specific to sciences dealing with purposive human behaviour seem so characteristic of history that we are not able to say whether they are terms from a presupposed science or specific to history itself. It seems clear that one part of these terms are not specific to history, namely, those which come from what is called "individual psychology". If anything, singular statements in individual psychology which appear in history books appear there because they may help explain social facts, or cultural facts.<sup>1</sup> But even if we agree that the terms which are terms of individual psychology are not specific to history, we still have to deal with all those terms that come from sciences of social behaviour.

The tendency of historians in the last century has been to stress the fact that history is a study of social or cultural phenomena. Some find the origins of this view in Guizot, Thierry, and Mignet; others emphasise Marx's decisive role in the formation of this approach; still others find it in the writings of historians like Lamprecht, and Pirenne; James Harvey Robinson has called it "The New History". This tendency, as we view it in the language of this paper, incorporates the admission that history presupposes all the social sciences. But, it might be thought, if history is to be a discipline distinct from the others, then, over and above what it takes from other sciences, it must contain terms that are neither economic, nor sociological, nor anthropological, etc. But if this condition is imposed, *i.e.* if all these sciences are regarded as distinct from history, and if the historian is expected to produce terms which fall into none of these sciences, it will be impossible for the historian to establish the uniqueness of his discipline. No examination of history books will yield terms that cannot be classified as terms from some other science. If one imposes this task on the historian in search

<sup>1</sup> E. Zilsel (*op. cit.*) says: "Psychological laws deal with the behaviour of human individuals, historical laws with large groups of individuals, . . . with cultures, states, nations, occupations, classes".

of terms specific to history, one cannot hope to define "historical statement" or "historical explanation" as different from other kinds of statements and explanations.

An actual study of the predicates which the historian regards as those with which he is especially concerned, will not permit any sharp distinction between them and what are commonly called sociological predicates.<sup>1</sup> If it is the purpose of the historian to give a picture of the social structure and development of a given society, then historical statements will not be distinguishable from sociological statements. The same holds for historical and sociological explanations.

The point may be illustrated by reference to the recent work of Prof. F. J. Teggart—*Rome and China* (1939). Prof. Teggart emphasises the need for a consideration of *classes* of events in order to establish what he calls "causation in historical events" and therefore "historical laws". In order to illustrate his methodological point he chooses the class of barbarian invasions of the Roman Empire between 58 B.C. and A.D. 107. The first result of his investigation—the chief feature of which is the comparison of events occurring in different places throughout the world at that time—is that every uprising between 58 B.C. and A.D. 107 on the European borders of the Empire had been preceded by the outbreak of war either on the eastern frontiers of the Empire or in the Western Regions of the Chinese. More specifically, Teggart claims, "whereas wars in the Roman east were followed uniformly and always by disturbances on the lower Danube and the Rhine, wars in the eastern T'ien Shan were followed uniformly and always by disturbances on the Danube between Vienna and Budapest."<sup>2</sup>

If we consider the term "barbarian invasion of the Roman Empire during the period 58 B.C. to A.D. 107", as designating a class of events, we must ask whether it is specifically historical in a sense which would prevent it from being a term of sociology. The class denoted by this term, of course, is a complex one. It

<sup>1</sup> Hempel, in the paper cited above, says: "The considerations developed in this paper are entirely neutral with respect to the problem of 'specifically historical laws'; neither do they presuppose a particular way of distinguishing historical from sociological and other laws, nor do they imply the assumption that empirical laws can be found which are historic in some specific sense" (p. 47). In the present paper I am not neutral; in fact, I think that the phrases "sociological term" and "specifically historical term" designate the same class of terms.

<sup>2</sup> F. J. Teggart, "Causation in Historical Events", *Journal of the History of Ideas*, Vol. 3, pp. 8-9 (1942). This article contains a useful summary of the results of Teggart's *Rome and China*.



is the result of performing logical operations on other classes ; it is a logical product of at least three classes. The first is the class of invasions, symbolically " I " ; the second the class of activities carried on by barbarians, " B " ; the third, the class of events which went on in the Roman Empire between 58 B.C. and A.D. 107, " D ". As a term, this could be replaced by the term " I . B . D ", where "." is the sign for logical product of classes. This new class-name means the same thing as " the class of barbarian invasions of the Roman Empire during the period 58 B.C. to A.D. 107 ". The reason for breaking the latter down into simpler components is to show that it is constructed out of terms which are, I think, sociological and physical. The term " I " could be said to be a sociological term, because it designates a class of group-actions ; the property of *being an invasion* is assigned to actions of masses of people. The term " B " is sociological for the same reason. The remaining term, " D ", is physical. The only remaining sign is the sign for class-product, which is logical.

The fact that a term in a supposedly historical statement like Teggart's can be constructed out of sociological, physical, and logical terms, shows that statements which are normally called historical might, without impropriety, also be called sociological. That the same could be done for most laws which are called historical or sociological is a hypothesis which I believe, but which I shall not try to prove at the moment. It suggests that whatever the terms which are specific to history are, they are not any different from those of sociology, where the latter is construed as the science of society.

The only difference which could be said to exist between sociology and history is not based on the descriptive terms they contain, but rather on the logical terms they contain. It might be said that sociology contains the generalisations of the singular statements that appear in history. Such a distinction, although it is one that is fostered and practised by many historians and sociologists, amounts to the schoolboy's distinction between algebra and arithmetic. Arithmetic, corresponding to history, is said to contain statements like " $5 + 4 = 4 + 5$ ", but not statements like "For every  $x$  and  $y$ ,  $x + y = y + x$ ". The latter is said to occur in algebra. But we know that such a distinction is artificial and arbitrary. It is a distinction between two sciences on the basis of the amount of logic they use ; for we know that the difference between the first and second statements is that the first involves the logical operation of quantification, whereas the second does not. We feel that sciences should



not be distinguished on the basis of the amount of logic they use. Similarly, for sociology and history. If they only differ in the way that algebra and arithmetic differ for the schoolboy, there is no serious philosophic problem. One must observe, however, that whereas the schoolbook's distinction between algebra and arithmetic may have some pedagogic value, the distinction between sociology and history is stultifying. It separates the finding of fact and the formulating of laws into two distinct disciplines.

## VI.

*Summary.*—A historical explanation is not an explanation of present facts in terms of past facts. The determination of the type of an explanation depends upon the terms which appear in it. Because of the great number of sciences presupposed by history, it is difficult to say which terms are specific to history. When we are able to present the terms which are specific to history we shall be able to define "historical explanation". We may be sure, however, that whatever these terms are, they will not be different from sociological terms. The current distinction between history and sociology is artificial and not based upon any distinction between the kinds of subject-matter terms they contain, but rather upon the amount of logic they use.

### III.—THE EXTRA-LINGUISTIC REFERENCE OF LANGUAGE (I.).

BY EVERETT W. HALL.

"The purpose of words, though philosophers seem to forget this simple fact, is to deal with matters other than words."—BERTRAND RUSSELL.

#### I. THE OBJECT LESSON.

Professor Albert G. Ramsperger<sup>1</sup> asks us to imagine a man with a prodigious verbal memory who has completely mastered a foreign-language dictionary but, perhaps unfortunately, has become so engrossed in the rules of verbal substitution in this foreign tongue that he has forgotten his own language and "thereby lost sight of the fact that the original purpose of words was to symbolize things". He might easily suppose that his linguistic proficiency is all there is to knowledge. I have experienced an actual case rather similar to this hypothetical one. A young chiropractor had suffered a stroke. His doctors gave little hope for his life, and predicted that, even should he live, his intellectual capacities would be permanently impaired. But his wife told me confidentially that she didn't believe them. She took me to his room. He was absorbed in a dictionary. "For hours every day," she said, "he is oblivious to everything but his dictionary. Doesn't that prove he is not losing his intellectual grasp?"

Even amongst logical positivists it is coming to be increasingly recognized that the nature of the relation between (empirical) language and the matters of fact to which it refers cannot be adequately understood by a mere analysis of language.<sup>2</sup> It is the primary purpose of the present paper to make this recognition more definite and to suggest how words can "mean" extra-linguistic facts.

But before turning directly to this problem we must note that only certain sentences (empirical sentences, protocol-sentences, object-sentences, sentences at zero-level) stand in any significant relation to matters of fact. Hence it might seem proper to ask, "What sentences are you concerned with in asking about the

<sup>1</sup> *Philosophers of Science*, pp. 96-97.

<sup>2</sup> See Ayer, *The Foundations of Empirical Knowledge*, § 9. Something of this is surely revealed in Carnap's shift of interest from *The Logical Syntax of Language* to an *Introduction to Semantics*. But cf. Part II of this paper.

relation of language to matters of fact?" Put generally, my answer is very simple: "I am concerned with sentences that assert something about wholly non-verbal states of affairs". But to become more specific leads into difficulties.

Let me take my departure from Russell's discussion of "the object-language"<sup>1</sup> in his very stimulating book, *An Inquiry into Meaning and Truth*. I feel that Russell's discussion is confused. This confusion is represented in his ambiguous assertion that words in the object-language "must not be such as presuppose the existence of language" (p. 77). This seems to mean a number of things. It seems to mean that object-words are genetically primitive—the first ones to be learned; they are "words which have been learnt without its being necessary to have previously learnt any other words" (p. 80). Again (and Russell seems to consider this the logical counterpart of psychological primitiveness), object-words are "words having meaning in isolation" (p. 80). This means, at least in part, that every object-word is such that it can function as a total sentence: "in the object-language . . . every single word is an assertion" (p. 92).<sup>2</sup> Finally, at least by implication, Russell means by 'object-language' any language whose designata<sup>3</sup> are wholly non-verbal. "In the secondary language [*i.e.* the language of next higher level than the object-language] we are concerned with words of the object-language . . . with the relation between object-words and object-sentences on the one hand, and what they designate or assert on the other hand" (p. 96). Here, then, we have at least three distinguishable uses of the terms, 'object-word' and 'object-language'. For convenience in reference, I shall speak of them

<sup>1</sup> Technically, Carnap's usage is more precise than Russell's. He calls any language an "object-language" relative to a metalanguage that talks about it. The lowest-level language, whose "designata" are non-verbal matters of fact, he speaks of as at the "zero-level". I shall follow Carnap's usage whenever precision seems to require it. I hope the ambiguity of 'the object-language' will not cause confusion.

<sup>2</sup> Another usage (Russell seems to think derivable from the preceding) is that object-words designate only sense-data. "Every single word of this (the object-) language is capable of standing alone, and, when it stands alone, means that it is applicable to the present datum of perception" (p. 92). Any thing-word, such as 'dog', involves a condensed induction and a tacit prediction; it therefore is really an abbreviation of a set of sentences combined by such logical connectives as 'and' or 'or', or of an existential sentence containing the logical-word 'some', and hence, on Russell's views, cannot be at the object-level. Cf. pp. 99, 189. Usually when Russell makes 'object-language' mean sense-data language, he is thinking of the epistemological desideratum of certainty.

<sup>3</sup> 'Designata' is a term I borrow from Carnap, stemming from Morris's terminology.

as the "primitivistic", the "isolationistic", and the "semantical" usages, respectively. This, I contend, conduces to confusion, and should be abandoned in favour of a single, consistent usage. Russell might well contend that though there is an intensional difference in these meanings of 'object-word', these meanings are extensionally equivalent, that is, they involve us in no confusion as to just what words are object-words, for every object-word has all the attributes required to make all these definitions appropriate.

To this I would object that it is not the case. A psychologically primitive word (that is a word which can be learned [*i.e.* appropriately used or responded to] without the necessity of having previously learned any other word) need not be a word which "can stand alone", or "designates non-verbal matter of fact". I have a dog whose education has been sadly neglected. She "knows" only two words: her name and 'no'. So far as I can determine, these are equally primitive in her development. 'No', I should suppose, is in this case the imperative form of 'not'. When I say 'No!' Topsy understands she must not do what she has started doing. Here is a negative that is psychologically primitive. But Russell has ruled out negatives from the object-language on the ground that they are equivalent to denial and hence presuppose sentences denied (*i.e.* they cannot stand alone) (p. 78, and *passim*), and really speak about words (*viz.*, the denied sentences which are claimed to be false) (p. 95). Thus 'no' in Topsy's vocabulary is an object-word in the primitivistic usage, but not in the isolationistic or semantical.

Similarly, I would argue that a word which is in the object-language in the semantical sense need not be so in the isolationistic sense. There are words (or other symbols) which cannot stand alone, as sentences, which yet function in sentences (in conjunction with other words) to assert wholly non-verbal matters of fact. Take predicates and relation-words. The objective 'red' cannot stand alone as an assertion, but 'This book is red' can, and this sentence is at the object-level semantically; likewise for 'on' and 'The book is on the table'. In general, in any language there are rules for the formation of sentences at the semantical object-level out of words (and other symbols) some of which, at least, cannot function in isolation as sentences in that language. In written English, for example, 'fire' is not a sentence, though 'Fire!' is. (Russell uses this example, but does not see that '!' and the change of 'f' to 'F' has conjoined 'fire' with other symbols to change the word into a sentence.)

More specifically, Russell says that such purely syntactical

words as 'is' and 'than' cannot occur in the object-language (p. 78). Now I admit that 'is' alone asserts nothing. But I submit that its occurrence in a sentence does not exclude that sentence from the object-language in the semantical sense. Russell's isolationism should force him to deny all syntax to the object-language. However, in a note he says, "There must be syntax, but it need not be rendered explicit by the use of syntactical words, such as 'is'" (p. 79); and apparently he feels that 'yellow (A)' is in the object-language, whereas 'A is yellow' is not, since the latter contains a *word*, 'is', which cannot stand alone. But this, it seems to me, involves a confusion. All sentences have syntax. There are sentences at the object-level. Whether their syntactical features occur as "words" or not is irrelevant. Obviously, however, the syntactical elements in a sentence cannot stand alone, as sentences (without vocabulary elements). That 'is' is called a word but parentheses are not, is an accident of English; in the two sentences above, their function is the same. The real issue is whether, on the isolationistic usage, syntax can be present in the object-language at all. Now in so far as isolationism destroys the difference between sentence and word at the object-level (p. 92) and claims that "every single word is an assertion", it would at least seem to rule out syntax at the object-level. If it does not, if the object-language has syntax, then there is no reason for ruling out syntax-words but allowing syntax symbols of other sorts (order, parentheses, etc.) at this level. In any case, my contention is the same: The presence of syntactical symbols (whether called "words" or not) in a sentence does not, itself, require that the sentence be about language; it may, in the semantical sense, be at the object-level. Thus there are sentences in the object-language in the semantical sense which are not at that level in the isolationistic sense (since containing words or other symbols that cannot stand alone as sentences).

But we must consider another meaning of 'object-word' in the isolationistic usage. Every object-word has meaning in isolation, we are told. This may mean that a word may have meaning (may designate something) even when not occurring as or in a sentence. If so, it may well be Russell's intention to assert that no syntactical symbols (and specifically no syntactical words) have extra-linguistic meaning in isolation. Therefore they are not included in the object-language in either the isolationistic or the semantical senses of 'object-language'.

Now I would agree that no syntactical symbol has extra-linguistic meaning in isolation. 'Is' or '... (- - -)', by itself,

designates no matter of fact. But this does not exclude it from the object-level in the semantical sense. For, 'is' in isolation does not designate a linguistic property. In isolation it is not a symbol at all. It only functions symbolically (as a "word" if Russell wishes) when in conjunction with other symbols in a sentence, and when it does occur in a sentence (and therefore is a word), neither it nor anything in the sentence, nor the sentence as a whole, need mean or assert any properties of language. Neither 'A is yellow' nor 'yellow (A)', where 'A' is a proper name for a certain surface, asserts anything about language; though neither '... is ---' nor '... (---)' designates any non-verbal matter of fact. 'Syntactical words in isolation do not designate matters of fact' does not imply 'Syntactical words (whether in isolation or *in situ*) designate verbal properties'. For, it is consistent with the first sentence in quotes above that syntactical words never designate anything, or that they designate matters of fact but only when *in situ*. Syntactical words may simply be indications for reading, *e.g.*, 'A is yellow' is to be read, The surface whose proper name is 'A' has the property yellow. Now this could be translated into a set of sentences, some of which are about language: *e.g.* " 'A' is the proper name for a certain surface", etc. Is this what 'is', as an indication of how to read a certain set of words containing it, *really* means? *I.e.*, is 'is' necessarily at a higher level than the objective, in the semantical usage? No. For any group of words forming a sentence must have some indication (syntax) of how it is to be read. It would follow that if such indications designate language-properties, there can be no sentence on the object-level semantically.

Parenthetically, this throws some light on the relativity of the concept, 'object-language' (or better, 'language at zero-level'). Any sentence may be translated into a set equivalent to it in truth-value, one of whose members, at least, is of the form "... ' (in the original sentence) means '---', and thus is at a higher semantical level than the original. *E.g.*, 'All my sons are ill' can be translated into the (equivalent) set: 'David is ill', 'Donald is ill', 'Richard is ill', and "'All my sons' designates David, Donald and Richard". Again, 'Here is my pen' can be translated into 'A cylindrical, brown patch is in a visual-location,  $l_1$ ', 'A smooth, hard tactile surface is at touch-location,  $l_2$ ', ..., etc., and "'My pen' designates a cylindrical brown patch related in certain ways to a smooth hard tactile surface" and "'Here' designates a correlation of  $l_1$  and  $l_2$  ..., etc." Likewise, 'Here-now green' can be translated into, 'A central

region of my present visual field has the colour exemplified by objects *a, b, c, d, . . .*, etc.' and "'Here-now' designates a central region of my visual field" and "'Green' designates the colour exemplified by the objects *a, b, c, d, . . .*, etc.'" There is thus no such thing as an absolute zero-level. ('The object-language must be a sense-datum language' is nonsense.) There are different zero-levels for different semantical language hierarchies. The failure to recognize this, particularly, the argument '*p*' is translatable into "'*p*' is true", therefore '*p*' cannot be at the object-level, I shall call "the semantical fallacy". Thus Russell's whole attempt (from a semantical standpoint) of finding the object-language is mistaken.

To return from this digression. I said that syntactical words may designate nothing (whether in isolation or *in situ*), but may be indications for reading. Another possibility (likewise consistent with the admission that syntactical words in isolation designate no extra-linguistic facts) is that syntactical words do designate matter of fact, but only when taken *in situ*. Consider, 'A is yellow' and 'B is blue'. The state of affairs that would make these sentences true is quite different from that which would make the following true, 'A is blue' and 'B is yellow'. Hence 'is' (in its copulative use) may designate a matter of fact, *viz.*, what properties are exemplified by *what* individuals, the differential exemplification of specific properties by particular individuals. Of course the use of 'is' is not the only linguistic means of doing this: '*. . . (- - -)*' might be used, or spatial or temporal proximity of the marks or utterances, 'A', 'yellow', etc. But in each of these there is a syntactical feature, and it does have a designative function at the object-level, *viz.*, designation of a differential attachment of properties to individuals.

Syntactical words (or symbols) may thus occur at the semantical object-level, though not at the isolationistic object-level (since in isolation they cannot stand as sentences nor designate extra-linguistic matter of fact).

I hope this sufficiently clarifies my contention that Russell's use of 'object-language' is confused. Next, and briefly, I want to indicate why I choose the semantical usage for 'object-language' and 'object-word'.

The objection to the primitivistic use of 'object-word' is that 'object-word' is thereby made relative to the individual; the question as to whether a word is an object-word or not would only have meaning relative to the development of a given individual's language habits. There may, of course, be certain discoverable laws about the formation of people's language

habits, but these must be discovered through investigation of particular instances, and this is in general the task of the psychologist, not the philosopher.

The isolationistic usage is unacceptable because, strictly, it implies the absence of sentences from the object-language; it tacitly reduces the object-language to a vocabulary. As I have argued, if the criterion of being at the object-level is capacity to assert or designate in isolation, then syntactical symbols are not at the object-level. Now of course it might be held that though syntactical symbols are not at the object-level, expressions, *viz.*, sentences, containing them are. But this seems ruled out by Russell's contention that all words at the object-level can function, in isolation, as sentences, and (apparently) that everything that can be said about matter of fact can be stated in such single-word sentences (*cf.* p. 94). Against this I contend that no single word in isolation can function as a sentence: 'fire' must become 'Fire!', etc.

The chief advantage of using 'object-language' in the semantical sense (besides avoiding the above disadvantages) is that it then functions to designate the lowest rung in a (fairly unambiguous) hierarchy. There may be an analogous hierarchy for the primitivistic usage of 'object-word' (*e.g.*, words of Class I can be learned without having first learned other words, those of Class II can be learned only after words of Class I have been learned, etc.), but if so, it would be relative to individuals, and at best its levels would be pretty vague. On the isolationistic usage, I do not see how more than two language-levels could be made out (the object-level and one other, composed of words that cannot stand alone).

Freedom from confusion in the usage of 'object-language' will help us in dealing with the problem of the status of logical words. Russell asserts categorically that logical words cannot occur in the object-language, and his arguments indicate that he is using 'object-language' in all three senses we have distinguished. Our concern will be as to whether they can occur in the object-language in the semantical sense. However, we might spend just a moment on the question whether logical words can occur in the object-language in the other meanings of this term. I have already indicated that it seems to me such a word as 'not' can be psychologically primitive. I can imagine a child, with greedy proclivities towards candy, being taught 'not all' before other words, or a child with poor eating habits being taught to respond to 'all' before he has learned other words (such as 'oatmeal'). It would be irrelevant to object that in such instances, 'not',



'all', 'not all', etc., implicitly presuppose other words (such as 'oatmeal', 'candy'), for the primitivistic criterion is simply priority in learning (not what is implicit or presupposed). So I conclude that in the primitivistic usage of 'object-language', the object-language may contain logical words (whether it does is determined by observation).

In the isolationistic usage, however, I would agree with Russell that logical words cannot occur in the object-language. Here, however, a distinction is important. When we ask whether a word can stand alone or have meaning in isolation, we may mean to ask whether people ever actually utter it and understand it in isolation. In this sense, I would say that logical words do occur as object-words (the cases in the preceding paragraph are instances). But we may mean to ask whether the language permits it, whether cases of isolated usage are not to be treated grammatically as ellipses. It is in this sense of "cannot" (*i.e.* not permitted by language-rules) that I would agree that logical words cannot stand alone, therefore cannot be object-words. Another way of putting this is to say that logical words are incomplete symbols.

My real concern is as to whether, in the semantical sense of 'object-language', logical words can occur in the object-language. In one sense, Russell's contention that logical words cannot occur in the object-language is the merest truism. This is the case if by definition 'logical words' refer to (certain) words which designate words (or sentences) or relations of words (or sentences). If, that is, we refuse to classify a word, in a given usage, as logical unless it can be shown in that usage to refer to words or word-relations, then we can be sure that no logical word can be an object-word. But no problem is solved in this way. We would have to determine whether 'some', 'all', 'not', etc., in various usages, *are* logical words.

If, however, we were to define 'logical word' by enumeration; if, *e.g.*, we were to make it synonymous (in English) with "'true' or 'false' or 'all' or 'some' or 'not' or . . .", etc., then it is not at all a truism that no logical word can be an object-word. Probably the best case can be made out for 'true' and 'false'. But even here we would have to be on our guard. Consider an infuriated woman accusing her mate: "You have been false to me". Here 'false' (or more accurately, the whole sentence—we need not now consider whether individual words have extra-linguistic meaning) may well be at the object-level. Russell might reply that what this woman is vehemently saying is simply, "'*p*' is false" where '*p*' is the name of a certain sentence, *viz.*, her husband's marriage vow. But women have been known to accuse

men of falsity when those men had uttered no marriage vow. And in any case, the woman accuses the *man* (not some sentence he uttered) of being false. It seems clear, then, that 'false' may refer to an observable, non-verbal property, such as the property designated by 'adulterous'. Now there is, of course, a simple reply Russell could make. He could say, "When I say that 'false' cannot be an object-word I am referring, by 'false', only to a property of sentences. The letters, 'false', may be used as other words, *e.g.*, as referring to a certain evil tendency on the part of husbands, but this isn't the 'false' I'm talking about." If he so replies, then no one will object to his innocuous contention: "When used to refer to properties of sentences, 'true' and 'false' cannot be object-words, *i.e.* words referring to wholly non-verbal matter of fact".

We meet graver difficulties with other logical words. Consider 'not'. Frequently 'not' is used as equivalent to 'false' ( $\sim p \equiv \text{" 'p' is false"}$ ). In such cases, of course, it is a truism that 'not' is not an object-word. But is 'not' always used in this sense? It seems to me not: not even, in fact, in logic. In the first place, " $\sim p \equiv \text{" 'p' is false"}$ " presupposes (or rather is a form of) the law of excluded middle, and so does not obtain in logical systems where that law does not obtain.<sup>1</sup> Even in such systems the negative is needed. What it means, it seems clear, is exclusion from a class (or absence of a property). Take a three-valued logic (true, doubtful, false). It must be possible in it to say, 'It is not the case that *p* is true' or ' $\sim p$ -true' (which would be equivalent not to '*p*-false' but to '*p*-false or *p*-doubtful'). Technically this distinction between 'not' and 'false' in a multivalued logic might be avoided as follows: ' $\sim p$ ' either not permitted or is equivalent to '*p*-false'; ' $\sim(p\text{-true}) \equiv \text{" 'p-true' is false"}$ '  $\equiv$  '*p*-false or *p*-doubtful'; ' $\sim(p\text{-doubtful}) \equiv \text{" 'p-doubtful' is false"}$ '  $\equiv$  '*p*-true or *p*-false', etc. But this, it seems to me, is simply an extensional formulation of the intensional significance of 'not', *viz.*, excluded from the class, . . . And this can also be urged for two-valued logics. Without classes of mutually exclusive symbols, logical systems are impossible.<sup>2</sup>

Now of course this meaning of 'not' ( $\equiv$  'excluded from') as occurring in logical systems is not at the object-level. But this is because the class or classes exclusion from which is expressed by 'not' are symbols (words, sentences, or their forms). The radical sense of 'not' (*i.e.* excluded from) can occur at the object-level.

<sup>1</sup> Russell himself points this out, Ch. XX.

<sup>2</sup> Cf. Charles Baylis' "Are Some Propositions Neither True nor False?" *Philosophy of Science*, 3, 1936, p. 156.

That 'not' can legitimately occur in sentences at the object-level seems to me established by the fact that object-sentences can be false. What is meant by saying that an object-sentence is false? <sup>1</sup> We may immediately rule out the possibilities: the object-sentence is self-contradictory and the object-sentence contradicts some other sentence (at object-level). For contradiction requires the occurrence of 'false', or some equivalent, and hence cannot occur at the object-level. It seems to me that the only legitimate answer is that for an object-sentence to be false means that it asserts something which is *not* the case, it asserts that a property or relation obtains in a situation where, as a matter of fact, it is absent. If so, then it is possible to change a false object-sentence into a true one by the introduction of 'not', and therefore 'not' (as asserting the absence of a property or relation from a specified region of fact) can occur at the object-level.

The only way to avoid this is to deny that there can be false object-sentences. This indicates, perhaps, one of the motives of those who claim that the object-language must be a sense-datum language. For the sense-datum theory of perceptual knowledge is basically an attempt to gain certainty, to avoid the possibility of error at the foundations of our empirical knowledge. If our object-language includes thing-sentences, then it includes tacit predictions, therefore possible error. If, however, it is restricted to sense-datum assertions, no error is possible. As Russell says, "When you have said 'that is a dog', subsequent events may astonish you; but when you have said 'that is white', nothing in your statement gives any ground for surprise at what happens next, or for supposing that you were mistaken in saying that what you saw was white" (p. 99). Such a view has a very interesting consequence, which Russell indicates: "So long as your words merely describe present experiences, the sole possible errors are linguistic, and these only involve socially wrong behaviour, not falsehood" (p. 99). Let me put it more strikingly: If the object-language contains no false sentences, then all false sentences must be about language. Furthermore, if, as some maintain, the only linguistic errors are those arising from breaking conventional rules of language (a factual error about actual language would be about matter of fact and thus at the object-level), it would follow that the only errors mankind is subject to are transgressions of linguistic etiquette. In fact, even these

<sup>1</sup> This anticipates the second part of this paper, dealing with the relation of object-language to fact, but it seems unavoidable in trying to answer our question as to what the object-language includes.

would not be errors, for when you break a language-rule the result is not a false sentence but nonsense. However, we must draw a distinction here. Suppose a rule of language is broken (*e.g.*, suppose ' $(x) \cdot \phi(x)$ ' is asserted in place of

$$' \phi(a) \cdot \phi(b) \cdot \sim \phi(c) \cdot \dots ', \text{ etc.})$$

Then, in a syntactical metalanguage one could assert that the utterance was linguistically legitimate. *This* assertion would be false. But only this sort of error could be allowed by those who deny that an object-sentence can be false. And what this really amounts to is that 'falsity'  $\equiv$  'contradiction'. For a sentence that broke a rule of language would not be false, but only a sentence which asserted that a sentence which breaks a rule of language does not break a rule of language would be false. (*E.g.*, replacing ' $\phi(a) \cdot \phi(b) \cdot \sim \phi(c) \dots$ ' by ' $(x) \cdot \phi(x)$ ' is not erroneous, unless the rule ' $(x) \cdot \phi(x) \equiv \phi(a) \cdot \phi(b) \cdot \phi(c) \dots$ , etc.' is accepted.) This results in the most vicious form of coherence theory of truth: *viz.*, that 'truth'  $\equiv$  'linguistic self-consistency'.<sup>1</sup>

Thus I maintain that 'not' can occur at the object-level. When it does, it asserts not the falsity of some sentence but the absence of some character from some region of fact. Such absence I take to be itself an ultimate aspect of matter of fact, not to be linguistic nor propositional (a matter of "propositional attitude", as Russell would say).

It might not be amiss here to digress long enough to indicate why I do not agree with another view which gives existential (extra-linguistic) significance to negation. I refer to a view of W. E. Johnson's concerning determinates under a common determinable (stemming from Aristotle's contraries).<sup>2</sup> This view holds there are existentially incompatible characters (properties and relations), such that if an existent exemplifies a certain character it cannot exemplify any other character in the set incompatible

<sup>1</sup> Thus an obvious consequence of denying false sentences at the object-level is the impossibility of true sentences at that level, unless 'true' and 'false' cease to be contradictions. For, if 'false'  $\equiv$  'inconsistency in linguistic usage', 'true' (as its contradictory) would  $\equiv$  'absence of inconsistency in linguistic usage'.

<sup>2</sup> A much more inclusive view of the significance of 'not' is that deriving from Plato, *viz.*, that 'not' signifies *any* difference or otherness. This is found in such statements as 'Redness is not sourness', 'Franklin Roosevelt is not Theodore Roosevelt'. But I am inclined to think that the designatum here is (in part) linguistic, and therefore that 'not' in this sense cannot occur at the object-level. Fully expressed, 'Redness is not sourness' means that 'redness' names a quality different from the quality named by 'sourness'. However, it may be that there are cases of 'not' in the sense of 'other than' which involve no verbal factor in their designatum; *e.g.*, 'That is not the man who robbed me'. If there are such cases, my position is strengthened. 'Not' can function as an object-word.

with it (if an area is yellow it cannot be blue or red or . . . , etc.). This view would say that negation, at the object-level, is really an ambiguous or non-specific assertion of an incompatible character, *e.g.*, 'A is not yellow' = 'A is blue or red or . . .'. This view seems to me untenable.<sup>1</sup> First, there is an objection raised by Russell (pp. 202-203). As regards colours, there would seem to be no question that they are existentially incompatible, but we do not have the same situation as regards sounds and odours. Yet negation as regards the latter seems to have as definite a meaning as in the case of the former. 'I do not smell any roses' would seem to be on a par with 'I do not see anything red'. But although the incompatibility view can take care of the latter (by exploring the visual field and finding in each part of it some colour other than red), it cannot deal with the former (for different odours may be compresent in the same olfactory region). I would add to this what seems to me a more basic criticism. Incompatibility is an empirical fact ('If the whole of a surface is red it cannot be yellow' is synthetic, not analytic).<sup>2</sup> Thus two qualities that are incompatible might have been compatible (*e.g.* red and yellow might have been existentially related as middle C and G above it). Therefore that two characters are incompatible must have been learned through experience, *viz.*, through observing that where one character is present the other is not (is absent). Hence the experience of absence (not-presence) is presupposed by the assertion of incompatibility. To put it differently. The statement that two characters are incompatible can be analysed into two statements: (1) the characters are different species under the same genus, and (2) a minimum existent exemplifying one does not exemplify the other. Thus clearly negation is more basic than incompatibility (since only part of the meaning of incompatibility).

Against the view that 'not' can occur at the object-level Russell has an interesting argument. He says it is impossible to observe the absence of anything. *E.g.*, if upon looking into the ice-box you say, "There is no cheese here", you must have *judged* in the case of each thing observed, "This is not cheese". You could not *see* that this butter is not cheese. So what you really mean by 'this is not cheese' is "'this is cheese' is false". You have at least an expectation, a propositional attitude (if not the sentence), 'this is cheese', and the 'not' asserts that this is false (*cf.* pp. 89-90).

<sup>1</sup> I accepted it at one time, *cf.* *The Philosophical Review*, 43, 1934, p. 345.

<sup>2</sup> I assume, without arguing the point here, that there are no *a priori* synthetic propositions.

But what does it mean to say that 'this is cheese' is false? It cannot be the same as 'this is butter' is true, for if you were looking at milk 'this is cheese' would be false, likewise. I cannot see how one can avoid admitting that for " 'this is cheese' is false " to be true requires that in the designatum of 'this' there is an absence of that set of observable properties peculiar to cheese. We may not psychologically *note* this absence unless we are looking for cheese, and thus are at least tentatively entertaining the proposition 'this is cheese'. But along with this we must also be able to directly experience the absence of cheese-properties, say in the butter, or " 'this is cheese' is false " would be utterly without empirical foundation. If this is the case, then *this* fact of experience (though it may never occur alone and without an accompanying propositional attitude) is legitimately expressed by a 'not' (in 'this is not cheese') at the object-level. Thus, in general, I hold that though it may be a psychological fact about our use of 'not' that it always accompanies and expresses a disappointed expectation, yet it has a designative function which is non-verbal and non-attitudinal; it refers to the absence of something (which as a fact we expected).<sup>1</sup>

Another objection against the occurrence of 'not' at the object-level is found in sentences of the form, 'I do not hear anything'. Such a sentence may be taken to be an assertion about everything in the universe (or in the universe of discourse), *e.g.*, an assertion that all sounds are unheard by me now or that everything in the universe is 'not a sound heard by me now' (*cf.* Russell, pp. 203-204 and 113). Since it is absurd to suppose I can now perceive everything in the universe or hear all sounds, it follows that 'I do not hear anything' cannot be a perception-sentence. Russell's interpretation of such a sentence is similar to his interpretation of 'this is not cheese'. *I.e.*, the 'not' expresses a propositional attitude—the rejection of a proposition previously held, say as an expectation (*e.g.*, I shall hear a motor-car). This would place the 'not' at a semantic (though not strictly a linguistic<sup>2</sup>) level above the object-level. But it seems to me no such interpretation is required. The sentence, 'I do not hear anything', can be taken simply to mean that sound is absent from my present

<sup>1</sup> We must also be on guard against the semantic fallacy. Since 'this is not cheese' is linguistically equivalent (extensionally) to " 'this is cheese' is false ", we may suppose 'this is cheese' cannot be at the object-level. But it would be just as legitimate to say, " 'this is cheese' is false " must be at the object-level, or to say, since 'this is butter' is linguistically equivalent to " 'this is butter' is true " to assert that 'this is butter' cannot occur at the object-level.

<sup>2</sup> Russell distinguishes sentences from the propositions they express. *Cf.* Part II. of this paper.

experience. It might be that I couldn't *note* this without a previous expectation of sound, but *what* is empirically noted is that a single region of fact (my present experience) does not contain sound, *i.e.*, sound is absent from that region.

So much for 'not'. It will be quite impossible in the present paper to discuss all logical words, or even all those discussed by Russell. I should like, however, to say something about one other, *viz.*, 'all'. My position concerning 'all' is essentially the same as that concerning 'not'. 'All' is often (especially by logicians) used to refer to sentences, and in such cases of course is not in the object-language. But even in such cases it is concerned not with sentences *per se* but with their relations to their designata, and therefore, ultimately, with matter of fact. And the matter of fact involved is of the sort often referred to by object-level sentences containing 'all' or an equivalent.

'All' is frequently used to assert that each of a set of sentences is true: ' $(x) \cdot \phi(x)$ '  $\equiv$  "' $\phi(a)$ ' is true and ' $\phi(b)$ ' is true and . . .". This usage places 'all' above the object-level, and it is perfectly legitimate, though sometimes rather strained, for it at least gives the appearance of ontological atomism. It seems a little strained to interpret 'all the air in the room contains carbon-monoxide' as meaning "The portion of air in the room designated ' $a$ ' contains carbon-monoxide" is true and, etc. But this ontologically atomistic suggestion can be avoided by the admission of the arbitrary character of the division of the designatum of ' $(x)$ ' into the designata of ' $a$ ', ' $b$ ', ' $c$ ', etc. However, there is one requirement that does not seem to be arbitrary: it is that of completeness. The designata of ' $a$ ', ' $b$ ', ' $c$ ', etc., must together completely make up the designatum of ' $(x)$ '. This is what I shall call the radical meaning of 'all' (as exclusion or absence was the radical meaning of 'not'). It is found formally in the requirement that for ' $(x) \cdot \phi(x)$ '  $\equiv$  ' $\phi(a)$ ' is true and ' $\phi(b)$ ' is true and . . .', to be true requires that ' $a$ ', ' $b$ ', etc., exhaust the range of values of the variable ' $x$ '.

Thus even in logic, 'all' means something more than "' . . . ' is true and ' . . . ' is true and ' . . . '". It means, as I have said, completeness of the logical summands. In logic, the completeness is a completeness of symbols and is determined by definition. But the same radical sense of 'all' is found in statements of fact, where the completeness is a completeness of non-verbal fact, and the completeness is itself a non-verbal fact, and therefore can be properly expressed by an 'all' at the object-level. Take, 'all the marbles in my hand are green' (I am supposing I have three marbles in my hand and they are all green). Now if one wishes one can translate this into "'marble  $a$  is green' is



true, 'marble *b* is green' is true, and 'marble *c* is green' is true". But such a translation would be incomplete; one would have to add, 'marbles *a, b, c* are all the marbles in my hand'. And to what does this last sentence refer? Clearly to an empirical fact of totality. And that this totality is an empirical fact is in no sense invalidated by the admission that we can designate it unambiguously only by an 'all' which is conjoined with two additional sorts of symbols, one indicative of the nature of the membership ('marbles') and one of its range or boundaries ('in my hand').<sup>1</sup> Thus 'all' is very obviously an incomplete symbol. On the isolationistic usage it couldn't possibly be an object-word. But in the semantical usage it can and does occur at the object-level. In fact I think it must in order that its occurrence at higher levels have significance. Suppose by definition 'all *x*' is defined as '*a, b, and c*'. Then the perceptive proposition "A set of marks similar to these, '*a, b, and c*', are all the marks to be found between the marks similar to these, '“ ”', in a certain region on this page (ordinarily designated, 'the preceding sentence')"—must be true; and it is at the object-level (it speaks of marks as visual items, not as linguistic elements).

It might be objected that '*a, b, c* are all the marbles in my hand' is true by definition, whereas '*a* is green' is true by correspondence to fact. But this would be purely arbitrary. One could just as easily reverse the matter and say '*a* is green' is true by definition or convention,<sup>2</sup> but '*a, b, c* are all the marbles in my hand' is true by correspondence with fact. The fact of the matter is that we do directly observe completeness or totality in many instances (of small finite aggregates), and we legitimately express this by 'all'. In such cases 'all' is as truly a descriptive relation-symbol as is 'precedes' or 'to the right of'.

Russell suggests another objection (pp. 321-322). He suggests that "all the marbles in my hand are green"  $\equiv$  '*a* is green, *b* is green, *c* is green'" is true if as a *fact* *a* and *b* and *c* are all the marbles in my hand, whereas I hold that to the right side of the above equivalence we must add: 'and *a, b, c* are all the marbles in my hand'.<sup>3</sup> I think that Russell's suggestion is wrong because

<sup>1</sup> When not expressed, this range may be the universe, or may be some smaller region indicated by the context.

<sup>2</sup> *E.g.*, by the convention that unless an object is green, we shall refuse to call it '*a*'.

<sup>3</sup> This may be unfair to Russell. What he actually says is, "... let us suppose that, in fact, A, B, C... Z are all the men there are, and let us suppose that there are occurrences correctly described as A's death, B's death, C's death, ... Z's death. Then, in fact, it is true that all men are mortal. Thus the number of occurrences required to insure the truth



it tries to put an accidental, actual truth-equivalence in place of equivalence as to reference or designation. Suppose as a fact that *a, b, c* are all green, then, to apply the principle at the basis of Russell's suggestion: "*'a, b, c, are all green' ≡ 'a is green'*" would be true for, as a fact, both sides of this equivalence are true. But clearly here the right and left members of this equivalence (although by hypothesis equivalent in truth-value) cannot be treated as equivalent in the matter of fact they designate or assert. So likewise for '*all the marbles in my hand are green*' and '*a is green, b is green, and c is green*'. In what is asserted, there is a completeness in former not present in latter.

I do not think that questions of size (finiteness, etc.) of aggregates are relevant to the present issue. It is a psychological fact that we directly observe completeness only in cases of rather small aggregates. We can only infer it for larger ones (essentially by correlations with symbols, whose completeness as observable marks is directly observed). The important point is that one can observe it, and that what is observed is expressed by '*all*', and '*all*' in such cases retains its radical meaning found also in cases involving reference to symbols.

So much for logical words. My contention is that if we take them as they occur, there is no general reason to suppose that they can never be at the lowest semantical level. '*True*' and '*false*', at least ordinarily, characterise sentences (in relation to matter of fact), and thus cannot occur in the object-language. But other logical words, such as '*not*' and '*all*', are not necessarily excluded from this lowest semantic level, and, in fact, have a radical meaning at higher levels to be found at the object-level.

But my whole line of argument is open to serious attack. The critic I have in mind will say that I have woefully confused "*semantics*" and "*pragmatics*".<sup>1</sup> He will say that I have

of '*all men are mortal*' is the same as the number of men, and no more. Other occurrences are necessary in order that we may know our list to be complete, but not in order that it may be complete" (p. 321). The trouble here lies in the ambiguity of '*number of occurrences*'. It may be taken distributively or collectively. Russell supposes that since, taking the phrase distributively, the completeness of a number of occurrences (whereby the number is the same as the number of some other class) is not itself another occurrence (thereby increasing the number), therefore there is no collective significance to the phrase, whereby, e.g., the completeness of a class may be an *aspect of* (but not a unit in) the number of the class. Apart from atomistic prejudices, I can see no objection to holding that aggregates have total properties as well as sets of members (with their individual properties).

<sup>1</sup> Cf. Morris, C. W.: *Foundations of the Theory of Signs*. Carnap has accepted this distinction, cf. *Foundations of Logic and Mathematics*, and *Introduction to Semantics*. In this last work, Carnap defines '*pragmatics*'

claimed a sentence to be at the object-level simply because it isn't obviously about language or wasn't intended to assert anything about language. We must, he would say, put aside such psychological considerations and ask whether a given sentence can be translated into a sentence or sentences in which there is reference to language. Only if it cannot is the sentence at the object-level. Or, he will say that our job (as philosophers) is to construct (arbitrarily) a language in which (1) there is no ambiguity as to semantical level, and (2) every sentence of ordinary speech and of science can be translated into one or more sentences in this constructed language.

In answer, let me say that I intend to investigate, in Part II of this paper, such a purely semantical language (Carnap's *Introduction to Semantics*), in so far as relevant to our problem. We shall find that our problem of the nature of the reference of zero-level expressions to extra-linguistic matter of fact cannot be dealt with by a pure semantics based on arbitrary rules of designation, for such a semantics is restricted to reference within a language, *i.e.* to reference where both sign and designatum thereof are expressions. I shall attempt to show that at the zero-level (but not at others) there must be elements serving both as symbols and as (in part) designata, through their conjunction, in their own occurrence, with other matter of fact. Thus pragmatics (concerned with the occurrence of symbols) is introduced into semantics (concerned with the designation of symbols).

This is anticipatory. In retrospect, perhaps I may be allowed a bit of white-wash. Surely, in this matter of confusing pragmatics and semantics, I have not sinned more than Russell. And if Russell has brought in psychological considerations to show how narrow the object-language must be, perhaps I may be forgiven for trying to widen the object-language by equally psychological considerations.

as follows: 'If in an investigation [of language] explicit reference is made to the speaker, or, to put it in more general terms, to the user of a language, then we assign it to the field of pragmatics' (p. 9). This, I think, is too narrow. Even should one use 'pragmatics' to refer to the investigation of any psychological factors in the use of language, I believe the usage would be too restricted. I suggest that 'pragmatics' refer to the study of the occurrence-aspects of language. Thus if technical symbols are not extensively used in symbolic logic because of cost-factors or because not reproducible by paper and ink, this would be a pragmatic consideration. I shall use 'pragmatics' in this wider sense.

(To be continued.)

#### IV.—DISCUSSION.

##### BERKELEY'S ESSAYS IN THE *GUARDIAN*.<sup>1</sup>

THE *Guardian* ran from Thursday, 12th March, 1713, to Thursday, 1st October of the same year. Steele, the editor, had made great friends with Berkeley, who came to London for the first time early in January, 1713. Berkeley is known to have been a contributor to the *Guardian*, and is said to have received from Steele a guinea and a supper for each paper he wrote. All the contributions were anonymous, and as Berkeley has left no statement on the subject, there has long been a doubt as to which essays were his. In the *Gentleman's Magazine* for March, 1780 (p. 125), a reviewer says of the article on Berkeley in the new *Biographia Britannica*, 'In the Life of Bishop Berkeley we should have been informed which of the *Guardians* were by him. No. 69, I know, was one.' The earlier collections of the *Works* contained none of these essays, and A. C. Fraser's edition (1871, vol. iii) was the first to include them. Fraser assigned fourteen essays to Berkeley, and these fourteen, being included also in Sampson's edition (1897-8) and in Fraser's later edition (1901), have come to be regarded as Berkeley's almost without question.

Fraser (1871, vol. iii, p. 143 n.) introduced the essays with the note, 'The fourteen Essays in the *Guardian* which are here reprinted are attributed to Berkeley upon external and internal evidence which, in the case of most of them, seems ample. *Guardian*, Nos. 3, 27, 35, 39, 49, 55, 62, 70, 77, and 126, are assigned to him by his son, Dr. George Berkeley, as well as by the annotators, who add to these Nos. 83, 88, 89. No. 69 is claimed for Berkeley in the *Gent. Mag.*' That note is far from satisfactory, and gives me the impression that Fraser did not study the question of their authorship for himself, but relied on the ascriptions contained in some of the later editions of the *Guardian*.<sup>2</sup> Some ascriptions vary from

<sup>1</sup> The following abbreviations are used :

T.V. *The New Theory of Vision*.

Princ. *The Principles*.

Pass. Obed. *Passive Obedience*.

Alc. *Alciphron*.

CPB. *The Commonplace Book*. The numbers are those in my edition ; Johnston's numbers are added in brackets.

<sup>2</sup> The following editions of the *Guardian* contain no ascriptions other than the indications given by *The Publisher to the Reader*, 1714, 1728, 1735, 1822 ; the editions of 1789 and 1793 (Moore's) have specific inscriptions, the former with lengthy notes about the authorship. Moore's edition simply gives the names of the supposed authors in the Table of Contents. The ascriptions in the 1789 edition do not entirely agree with those in Moore's edition. In Moore's edition Nos. 69 and 81 are assigned

edition to edition, and some were clearly made on slender grounds. For instance, the 1789 edition has the naïve statement on No. 10, 'In this edition all Papers are ascribed to Steele that could not upon good authority be assigned to any other writer. The Annotator nevertheless suspects that No. 10, and many more attributed here to Steele, were written by other hands.' Sampson, though he follows Fraser's lead, bluntly says (*Works*, vol. ii, p. 2), 'No essays can be assigned to Berkeley with any certainty . . .', and Jessop's *Bibliography*, listing the fourteen essays, says, 'The ascription of all these to Berkeley is not certain and needs to be reconsidered'.

I have read *ad hoc* all the 175 essays which form the *Guardian*, and I have very carefully examined the fourteen attributed to Berkeley, and I have come to the conclusion that twelve of these are his, and that the other two are not his. I do not think that any of the remaining 161 essays are by Berkeley. The net result of my study is, (1) to exclude from Berkeley's *Works* Nos. 3 and 69, which in Fraser's editions are numbered I and VIII and headed, Remarks on Collins' DISCOURSE OF FREE-THINKING, and Fénelon's DEMONSTRATION, respectively, and (2) to confirm the Berkeleian authorship of the remaining twelve essays contained in Fraser's editions.

I will now state the evidence for these conclusions, beginning with the case for rejecting Nos. 3 and 69.

I reject No. 3 because Steele claims it for himself, and because it is not the sort of thing that Berkeley would write.

A motion was brought before the House of Commons for the expulsion of Steele from the House on the ground of unpatriotic writing. Steele pleaded his case, but the motion was carried. Shortly afterwards (1714) Steele published his account of the proceedings in his *Apology for Himself and his Writings*. The *Apology* is a lengthy statement of what he said or intended to say on that grave occasion. He represents himself as having then read to the House a long extract (the first third of the number) from the *Guardian*, No. 3, as evidence of his loyalty to the established religion, and the extract is given *in extenso* in the *Apology* (p. 44), and opposite the quotation is inset in the margin, 'Written by Mr. Steele himself'. The words were not added by a later editor; for I quote from a first edition, and Steele expressly says that he writes in the third person to avoid the tedious repetition of the 'I'. It is incredible that on such an occasion, when his political life was at stake, or in a subsequent record of what then took place, Steele should, either by inadvertence or deliberately, claim as his own the work of another.

Further argument is not really necessary, but there are several other reasons against the Berkeleian authorship.

to Steele, and its list of Berkeley's essays is the same as Fraser's except that No. 101 is assigned to Berkeley by Moore and that No. 69 is not. No. 101 is certainly not by Berkeley, see below, p. 263.

(1) Is it likely that Steele would entrust to an untried hand the third number of his new venture ?

(2) The opening words, 'I am diverted from the account I was giving the Town of my particular concerns . . .', are a plain and natural reference to the previous paper in which Steele began 'to explain who I am myself that promise to give the Town a daily Half-sheet'. From Berkeley's pen the words would mean nothing. It is just possible, I suppose, that the words might be a device for keeping up an appearance of continuity of authorship; but the literary fiction would be laboured, pointless, and out of keeping with the *Guardian's* practice. I have not noticed a case of one contributor pretending to be another. Besides, if Berkeley wrote the words, he must have done so *after* seeing Essay No. 2, of Friday, 13th March; i.e. he must have written or adapted the article on the Friday, and sent it in to Steele, who must have read it and seen it through the press also on the Friday, in order to have it in shape for the issue of the following day. Such hurried writing and hurried printing, even if possible then, would scarcely suit the leisurely methods of those days.

(3) A blunt soldier, like Steele, might say (without meaning it) that Collins 'deserved to be denied the common Benefits of Air and Water'; but Berkeley did not say what he did not mean. At times he is severe on free-thinkers as a class; but he usually tries to reason with them, and convince them; sometimes he impugns their motives, but on the whole he treats them as narrow-minded, misguided men; and I cannot think that he would have said this brutal thing about an individual, their leader.

(4) There runs through the piece a layman's argument which few clergymen would use, viz. that the free-thinkers are fools in that they do not have a really good time on earth, nor take advantage of the licence to which their principles entitle them. The free-thinkers, the essayist suggests, 'serve the devil for nought'. That is Steele, not Berkeley. It is a boomerang argument which, in the name of religion and morality, might easily provoke to evil. Berkeley would have disdained to use it. A man of his exact habit of thought and high principle would never have written the pungent, but unprincipled words, 'Would it not be a matter of mirth to find, after all, that the heads of this growing sect are sober wretches who prate whole evenings over coffee, and have not themselves fire enough to be any further debauchees than merely in principle'.

How, then, did this essay come to be attributed to Berkeley? The only reason for doing so, so far as I can trace, is that it is included, or is said to be included, in the list of his father's essays made by his son, Dr. George Berkeley. There are omissions in the list, and, as I show below (p. 252), it cannot be taken as decisive. If the son did assign No. 3 to his father, he was mistaken. He may have been led into the mistake by the assumption that since all his father's essays in the *Guardian* attack the free-thinkers,

his father had been placed by Steele in charge of that campaign, and was therefore likely to have fired the first shot.

The assignment of No. 3 to Berkeley was queried long ago, and in *The British Essayists* (1802), vol. xvi, p. xxiv, Chalmers writes of it, 'It is positively claimed by the Bishop's son as one of the ten papers his father wrote, but in Steele's *Apology* an extract is given from this paper, and it is said in the margin that Steele was the author. I know not how to reconcile these accounts; there is certainly nothing in it that Steele might not have written, and the express evidence of his *Apology* may be allowed to preponderate; on the other hand, the sentiments and manner of this paper seem connected by strong resemblance with Berkeley's general mode of treating the subject.' I dissent from the opinion that 'the sentiments and manner' are like Berkeley's, and I suggest that Chalmers' word 'preponderate' shows that he really thought that Steele wrote the essay.

I come now to No. 69 on Fénelon's *Demonstration*. This is assigned to Steele in the 1789 and the 1793 (Moore's) editions of the *Guardian*. I think Steele wrote it, but am not concerned to assert that he did so. My case is simply that Berkeley did not write it.

In the first place, why should we assign it to Berkeley? It is not among the ten assigned to him by his son, and the assignment rests solely, as far as I can find out, upon the undocumented, bald statement by the anonymous reviewer ('Crito') in the *Gentleman's Magazine*, quoted above, p. 247.

If the case for assigning it to Berkeley is weak, the case *against* is strong, though complex. There is here no one compelling argument, as in the case of No. 3; but there is instead a network of arguments with a cumulative force, the full effect of which is felt when No. 69 is read along with others in the series. Here are the chief points.

(1) It begins with a 'Precaution', which on the face of it is by an editor and not a contributor. A translation of Fénelon's book has been received, and naturally the editor takes notice of it. Besides, if Berkeley had written the 'Precaution', the words, 'Looking over the letters of my correspondents', would be out of place; they come naturally from the editorial pen.

(2) It is just possible, I suppose, that the Introduction is by Steele and the letter by Berkeley; but that supposition would make the construction of the paper, which in any case is slovenly, incredibly bad; for we should have to suppose that Berkeley sent in an unfinished and unsigned letter. The other essays, *viz.* Nos. 35 and 39, which Berkeley writes in letter form, are carefully constructed with address and signature, and they are addressed, not 'To the *Guardian*', but to 'Nestor Ironside Esq.' and 'Mr. Ironside', respectively.

(3) The writer commits himself (by calling it a 'just expression') to the opinion that the existence of God is 'the only thing of which we are certain'. Berkeley could not, I think, have done so.

(4) In the prayer itself occur the words, 'What is life in the ignorance of Thee? A dead, unactive piece of matter . . .'. Berkeley was known as an immaterialist; he had published one book against matter three years since, and a second book against matter just a fortnight before the date of this paper. (15th May, 1713, is the date assigned to the publication of the *Three Dialogues* in the 1789 edition of the *Guardian* in a note on No. 55.) It is not likely that he would have given publicity to a sentiment assuming the existence of matter. Were he the author of the paper, he could easily have omitted the offending words; for though they are attributed to Fénelon, the writer takes some responsibility for them, saying that he has not confined himself 'to an exact version from the original', but has tried to express the spirit of it 'by taking the liberty to render his thoughts in such a way as I should have uttered them, if they had been my own'.

(5) Whoever wrote No. 69 wrote also No. 81. No. 69 refers forward to No. 81, and No. 81 refers back to No. 69. The two are a pair united by style and content; yet no one has assigned No. 81 to Berkeley. I see nothing in No. 81 at all characteristic of Berkeley; its attitude to death and the future life is morbid, and has little or nothing of that sunny hope which marks Berkeley's utterances on those subjects.

(6) Nos. 70, 77 and 83 form a trio of great papers, undoubtedly by Berkeley, like one another in tone and content and design. (Nos. 77 and 83 are so closely united that they could be fairly called one paper spread over two issues.) If Berkeley wrote also Nos. 69 and 81, then he wrote five papers in a little over a fortnight—which seems unlikely. But suppose he did write them all, can there be any reasonable explanation of the order of the five? How can the pair 69—81 be fitted in with the trio 70—77—83? Why should the sequel to 69 be deferred so long, and why should this great trio be interrupted by the dull and unimportant No. 81?

I pass to the positive side of my thesis. Fourteen essays are printed by Fraser as by Berkeley. I have argued that two of these are not his. I have now to show that the remaining twelve *are* his. I do not know of any other systematic attempt to do so, and it is especially incumbent on me to make the attempt, because in rejecting No. 3 I have had to discredit, in a measure, what I believe to have been the main ground for the traditional ascription of any of the essays specifically to Berkeley, *viz.* his son's list. I propose to examine the essays one by one, but before doing so, I must place before the reader certain pieces of general information which bear directly on the problem.

That Berkeley was a regular contributor to the *Guardian* is beyond question. In the first (1714) collected edition of the periodical, apparently put out by the publisher after a quarrel with Steele, *The Publisher to the Reader*, after indicating or naming several of the contributors in these volumes, says, 'And Mr. Berkley of



Trinity-College in Dublin has embellished them with many excellent Arguments in Honour of Religion and Virtue'.

Again, Stock's *Life*<sup>1</sup> of Berkeley says, 'In February, 1713, he crossed the water, and published in London a further defence of his celebrated system of immaterialism, in *Three Dialogues between Hylas and Philonous*. . . . Two gentlemen of opposite principles concurred in introducing him to the acquaintance of the learned and the great: Sir Richard Steele and Dr. Swift. He wrote several papers in the *Guardian* for the former. . . . In point of fact Berkeley crossed the water early in the New Year; for on 26th January (Rand, *Berkeley and Percival*, p. 104) he wrote to Percival from London, and in the same letter he mentions that he had already received attention and hospitality from Steele, and he speaks of Collins' 'very bold and pernicious book entitled a "Discourse on Free Thinking"'. His letters to Percival of 23rd February and 27th March show him on intimate terms with Steele, and in that of 7th March he writes, "You will soon hear of Mr. Steele under the character of the 'Guardian'; he designs his paper shall come out every day as the 'Spectator'".

I must now deal with the tradition, accepted generally by writers both on Berkeley and on the *Guardian*, that his son, Dr. George Berkeley, assigned to his father ten essays, viz. Nos. 3, 27, 35, 39, 49, 55, 62, 70, 77 and 126. I have not succeeded in tracing the tradition to its source; but apparently the alleged statement was contained in a private letter; for the 1789 edition of the *Guardian* which often refers to the statement, has the following note on No. 77, 'This paper, No. 77, is ascribed to Bishop Berkeley, on the authority of his son . . . MS. letter'. The question for us is, What value should we attach to this traditional list? My reply would be that it should not be ignored nor treated as decisive. It is clearly right with regard to the majority of the ten essays; but, on the other hand, it assigns to Berkeley one essay (No. 3) which he did not write, and it fails to ascribe to him three essays (Nos. 83, 88, 89) which he did write. The list is right about nine essays, wrong about one essay, and deficient. Dr. Berkeley was a first year student at Oxford when his father died there; he would have been rather young to have received direct information from his father on the subject. His mother may have known, and have told him; or he may have found information in his father's books; but I do not think he could have studied the question at all carefully for himself; had he done so, he could not have failed to see that No. 83 goes along with No. 77, being in fact a continuation of it. Therefore the omission of No. 83 is a serious blot on the list. The reasonable course, adopted in my own investigations, is, I suggest,

<sup>1</sup> The first considerable *Life*, published in 1776. It later appeared in the *Biographia Britannica* (1780, vol. ii, pp. 247 ff.), for which it had been written (see Introduction to the 1777 ed. of the *Life*), and was also prefixed to the first collected edition of Berkeley's works (1784).



to accept the son's list as a basis for critical discussion, and if internal or other evidence confirms it, to accept the paper as Berkeley's, but not to regard what the list says or does not say as overriding evidence.

Here is a minor point, of possible interest to some readers. I have allowed myself to wonder why Berkeley after several papers in quick succession, is almost silent. After No. 89 (23rd June) he wrote, it seems, only one more paper, viz. No. 126 (5th August). This silence was due in part, no doubt, to Berkeley's two months' visit to Oxford. He went there about 20th June, and returned thence to London on 26th August (Rand, *Berkeley and Percival*, pp. 121 ff.). Possibly, too, Steele's sudden incursion into politics by his article on Dunkirk on 7th August (No. 128) made Berkeley unwilling to contribute further. He remained in London till 25th October, and from his letter to Percival of 2nd October, we see that he was still in touch with Steele at the *Guardian's* demise.

The following letter, whether it is by Berkeley or not, should certainly be mentioned or quoted in any study of his essays in the *Guardian*; for it strikes the keynote of his series of papers on the free-thinker. The letter is dated 16th March and it appeared in the *Guardian* of Saturday, 21st March.

SIR,

By your paper of Saturday last you give the Town hopes that you will dedicate that day to religion. You could not begin it better than by warning your pupils of the poison vented under a pretence to Free-thinking. If you can spare room in your next Saturday's paper for a few lines on the same subject, these are at your disposal.

I happened to be present at a publick conversation of some of the defenders of this *Discourse of Free-thinking* and others that differed from them; where I had the diversion of hearing the same men in one breath persuade us to freedom of thought, and in the next offer to demonstrate that we had no freedom in anything. One would think men should blush to find themselves entangled in a greater contradiction than any the *Discourse* ridicules. This principle of free fatality or necessary liberty is a worthy fundamental of the new sect; and indeed this opinion is [?] of an evidence and clearness so nearly related to transubstantiation that the same genius seems requisite for either. It is fit the world should know how far reason abandons men that would employ it against religion; which intention, I hope, justifies this trouble from,

Sir, Your hearty well-wisher,  
MISATHEUS.

This letter is exactly in Berkeley's manner; the signature voices his main contention, viz. that free-thinking is disguised atheism. He refers elsewhere to his personal knowledge of the free-thinking

clubs of London, see the *Advertisement of Alciphron*, *ib.* ii. 21, *Defence of Free-Thinking*, 7, and especially Chandler's *Life of Johnson*, p. 57, quoted in Fraser, *Works* (1901), vol. ii, p. 23, n. For 'free fatality', etc., cf. Alc. ii. 25, 'this curious piece of clock-work, having no principle of action within itself, and denying that it hath or can have any one free thought or motion, sets up for the patron of liberty, and earnestly contends for free-thinking', and for the reference to transubstantiation see Alc. ii. 26 on 'their tendency towards popery and slavery'. If 'Misatheus' is Berkeley, as seems to me highly probable, then the opening words of the letter confirm my rejection of the Berkeleian authorship of No. 3.

I have now to assemble the evidence for the Berkeleian authorship of the twelve essays. I have starred those contained on the son's list; but it will be seen that I am far from resting the case for all or any of them upon that list. I have taken the essays separately and studied them as with a microscope, and I have arranged the evidence so that readers can, if they wish, look up the references, examine the parallels and weigh the evidence for themselves. But here I call attention to two broad facts which emerge from this mass of detail. Several essays are indissolubly linked together by style, contents, and direct reference, and if you assert or deny that one is by Berkeley, you must be prepared to assert or deny the same of the others in the group. Thus No. 27 unquestionably carries with it Nos. 35, 39, 49 and 55. The other broad fact is the close connexion between these essays and the *Alciphron*. I daresay this connexion has been noticed by others; for me it has been a discovery, resulting from this present study. The connexion covers general approach and main lines of argument and extends to details and to words and phrases. Whoever wrote the *Alciphron* (one might fairly argue) wrote also the great majority of these twelve essays, and perhaps that argument might extend to all of them. I can well imagine that when Berkeley was writing that great work under the 'Hanging Rocks' on the strand at Newport, he had a copy of the *Guardian* at hand. These *Guardian* essays against free-thinkers must not be judged as casual pieces of journalism without internal connexion; they are the products of an acute, trained, and comprehensive mind, seriously directed towards a great contemporary movement of unbelief, and they subsequently took systematic shape in *The Minute Philosopher*.

Evidence that Berkeley wrote the following essays.<sup>1</sup>

\* No. 27. Saturday, 11th April. THE FUTURE STATE.

The argument for a future state from teleology and *ab appetitu immortalitatis* is that of a trained philosopher. The reference to 'the late glorious festival' (Easter) is a clerical touch.

<sup>1</sup> The \* means that the essay is one of the ten said to have been assigned to Berkeley by his son.

'a connexion, an adjustment, an exact and constant order'; cf. 'steddiness, order and coherence', Princ. 30, and 'a certain order and connexion', *ib.* 64, cf. *ib.* 151, etc.

'corporeal world' and 'sensible world', not material world.

'our free-thinkers, out of their singular wisdom and benevolence to mankind'; cf. 'enemies to the peace and happiness of the world', No. 83.

For the quotation from Plato cf. No. 70; for Plato as defender of the future state cf. 'The wise heathens of antiquity'; No. 55 *ad fin.*

'poverty of the imagination and narrowness of soul'; cf. 'narrower than ordinary', No. 39 and Nos. 70 and 77 *passim*, and Alc. i. 10.

'suppose a person blind and deaf from his birth'; a development of the Molyneux Problem, for which see CPB. and T.V. *passim*, and Princ. 43.

'eye hath not seen, nor ear heard'; a favourite text of Berkeley's, of which he makes philosophical use; it was to him scriptural proof of his principle 'significant words without ideas'; cf. *Draft Introd.* in Fraser (1901), vol. iii, p. 375, Alc. vii. 10, *Guardian*, No. 89, and his sermon on Life and Immortality (11th Jan., 1708).

'any new inlets of perception'; a similar argument appears in the above-mentioned sermon about our present 'dull inlets of perception'; cf. 'a few miserable inlets of sense', No. 70, and 'my own few, stinted, narrow inlets of perception', Princ. 81.

'a certain ingenious foreigner'; cf. the conclusion of No. 39. Nos. 27 and 39 (and No. 35 goes closely with No. 39) are thus clearly proved to be by the same hand, and there can be no manner of doubt that the hand was Berkeley's.

'very much out of humour in a late fit of sickness'; cf. Alc. ii. 17.

\* No. 35. Tuesday, 21st April. THE PINEAL GLAND.

\* No. 39. Saturday, 25th April. THE PINEAL GLAND OF A FREE-THINKER.

These two, for our purpose, may be treated as one essay; for they are united by the pen-name 'Ulysses Cosmopolita', by mutual reference, and by the conceit of the gland. The passage on M. Deslandes' sickness links the pair to No. 27, as a glance at the essays shows.

Conceits such as the Molyneux Problem and the Solitary Philosopher are often used by Berkeley in the CPB. as an aid

to philosophical discussion, and it would come natural to him to visualise the pineal gland, the Cartesian point of interaction, and make this semi-serious application of the conceit. The detail is worked out with philosophical correctness, and, I think, only a philosopher trained in mathematics, as Berkeley was, could write, 'One while, to trace a theorem in mathematics through a long labyrinth of intricate turns and subtleties of thought; another, to be conscious of the sublime ideas and comprehensive views of a philosopher . . .'.

For the description of the 'man of pleasure' and the reaction from sensual excess see Alc. ii. 17, 18.

For the narrowness of the free-thinkers see note on No. 27.

'certain men in black, of gigantick size, and most terrible forms . . . a tower filled with racks and halters'; cf. 'the black tribe', Alc. iii. 15, and 'that fierce and bloody animal an English parson', *ib.*, v. 30, and i. 3, the clergy would 'turn the world into a dungeon and keep mankind for ever in chains and darkness . . . a monster or spectre made up of superstition and enthusiasm, the joint issue of statecraft and priestcraft, rattling chains in one hand, and with the other brandishing a flaming sword . . .'.

'in the center . . . Atheism'; cf. *ib.*, i. 9, 'Atheism . . . is the very top and perfection of free-thinking.'

Degeneration of the term *free-thinker*; cf. *ib.*, i. 10.

\* No. 49. Thursday, 7th May. PLEASURES, NATURAL AND FANTASTICAL.

Note the conclusion 'as I have frequently taken occasion to animadvert on that species of mortals (free-thinkers), so I propose to repeat my animadversions on them . . .'—a clear reference to past and future papers dealing with free-thinking, and therefore firmly placing this essay in the series of those by Berkeley. All Berkeley's essays deal with this movement in greater or less extent, and after the opening one by Steele (No. 3) only three other essays (Nos. 93, 130 and 169) deal with it.

The main lines of this paper, theistic hedonism, teleology, and the contrast between sense and reason are characteristic of Berkeley's philosophy and ethics; cf. *Pass. Obed.*, 5, 7.

For the distinction between natural and fantastical pleasures see CPB. 144 (146), 773 (786), 787 (800), 852 (864), Alc. ii. 14, 16, and *ib.*, i. 9, 'these real natural good things, which include nothing of notion or fancy'.

The conceit of 'having a natural property in every object that administers pleasure to me', so finely exemplified, is not merely

an expression of philosophical detachment, I think, nor of solipsism, but is a characteristic utterance of theistic immaterialism. It may be compared with his advice to Percival (Rand, *op. cit.*, p. 58), 'there is nothing else wanting to complete your happiness, so much as a little more satisfaction in your own company'.

'a desire terminated in money . . . of outward distinctions', Berkeley's contempt of wealth and place was often noticed by his contemporaries.

Note 'my couches . . . are of Irish stuff, which those of that nation work very fine'.

'rob virtue of her support', developed in No. 55; *cf.* 'to lessen the motives to virtue', Alc. iii. 16.

\* No. 55. Thursday, 14th May. VIRTUE AND ITS SANCTIONS.

Showing the inadequacy of the Moral Sense theory without rewards and punishments. This essay is adumbrated at the end of No. 49, and is expanded in the third dialogue of *Alciphron*. It is linked with No. 49 also by the contrast between 'solid natural happiness' and 'titles, estates and fantastical pleasures'. Nos. 49 and 55 are the targets of witticisms by the writer (? Steele) of No. 58.

'gaining some greater future good'; Berkeley's theistic hedonism, see Pass. Obed. 5, 7.

'a set of refined spirits, capable only of being enamoured of virtue'; *e.g.* Shaftesbury and the upholders of the 'moral sense', *cf.* Alc. iii. 6, 'the refined moralists of our sect are ravished and transported with the abstract beauty of virtue', and *ib.*, iii. 12, 'those heroic infidel inamoratos of abstracted beauty'.

'the bulk of mankind who have gross understandings, but lively senses and strong passions'; *cf.* Alc. iii. 12, 'the many, who have quick senses, strong passions, and gross intellects'.

'virtue is her own reward'; *cf.* *ib.*, iii. 12, 13.

'The wise heathens of antiquity . . . by fables and conjectures'; *cf.* No. 27.

'future state . . . brought to light by the gospel'; a favourite text of Berkeley's, see his sermon on Life and Immortality, and No. 83.

\* No. 62. Friday, 22nd May. PUBLIC SCHOOLS AND UNIVERSITIES.

'a secret unenvied pleasure' and 'the scale of pleasure' as in No. 49; for the latter *cf.* Alc. ii. 14, 15, and Pass. Obed. 5.

'Beaus', *cf.* Alc. ii. 19.

The cultivation of learning motivated by religion and carried on through 'dead languages'; cf. Alc. v. 23—a close parallel. 'proportionable', a favourite word with Berkeley; see Nos. 35, 70, CPB. 569 (574), Princ. 73, and elsewhere.

For Berkeley's high opinion of universities, see Alc. v. 23; he was to visit Oxford in four weeks' time, and he described it as 'the most delightful place I have ever seen . . .'. Rand, *Berkeley and Percival*, p. 121.

The barbarism of northern nations before Christianity, cf. No. 88.

For the Parthian shot at the free-thinkers, see Nos. 27, 35, 49, 77, 89, 126; for their 'short views and narrow capacities' see Nos. 27, 70, 77.

\* No. 70. Monday, 1st June. **NARROWNESS OF FREE-THINKERS.**

This essay has the same general *motif* as No. 77, which in turn is intimately connected with No. 83.

For the narrowness of free-thinkers see Nos. 27, 39, 62; in No. 39 the cure prescribed was mathematics and religion; here it is philosophy, astronomy, and religion; for the comparison to the fly, cf. Berkeley's explanation of his term *minute philosopher*, Alc. i. 10.

'largeness of mind', cf. Princ. 105.

'the dust of a balance, the drop of a bucket, yea less than nothing', quoted also in the sermon on Life and Immortality, and in Pass. Obed. 6.

'proportionable', see note on No. 62.

'The greatness of things is comparative', cf. CPB. 204 (210), and Princ. 11.

'a few miserable inlets of sense', see note on No. 27.

'branched out into new faculties . . . this short span of duration', cf. the sermon on Life and Immortality, 'proportionate to what our faculties shall be . . . this span of life, this moment of duration'.

\* No. 77. Tuesday, 9th June. **THE SHORT-SIGHTEDNESS OF CRITICS AND MISERS.**

No. 83. Tuesday, 16th June. **THE SHORT-SIGHTEDNESS OF FREE-THINKERS.**

These two are one essay spread over two issues; they are united by mutual reference and common plan. The son's list of his father's essays contains No. 77, but omits No. 83. 'short-sightedness in the mind', cf. CPB. 742 (755), Princ. Introd. 5.

'two sorts of Goods', *cf.* CPB. 852 (864).

'relative goodness', *cf.* CPB. 569 (574).

'Profit and pleasure', *cf.* CPB. 541-2 (546-7); Alc. iii. 4.

'words and money . . . marks of things'; for *words* see Princ. Introd. *passim*, and Alc. vii. 13; for *money* see *Querist*, 23.

Happiness, social and personal, man's true aim, *cf.* Nos. 49, 70, and Pass. Obed. 7 ff.

Free-thinking destructive of human happiness, *cf.* No. 27.

'giants and monsters', *cf.* No. 39 'the terrible Giants in black'.

'these knight-errants', *cf.* Alc. vii. 23 'these philosophical knight-errants'.

'the mind of man is corporeal'; *cf.* 'corporeal spirits', No. 39, and Alc. ii. 25.

'religion is a state-trick'. The same words occur in Alc. ii. 25, *cf. ib.*, i. 7.

'Life and immortality brought to light by the Gospel' see note on No. 55.

'mediocrity of understanding', *cf.* No. 39.

The free-thinkers' quest of liberty leads through licence to slavery; *cf.* Alc. ii. 26.

No. 88. Monday, 22nd June. THE CHRISTIAN CONCEPTION OF GOD.

No. 89. Tuesday, 23rd June. THE CHRISTIAN CONCEPTION OF A FUTURE STATE.

The main authority for assigning Nos. 88, 89 to Berkeley is the following letter which Steele wrote to their author in No. 90, prefacing the letter with the words, 'I shall end this paper with a letter I have just now written to a gentleman whose writings are often inserted in the *Guardian* without deviation of one tittle from what he sends me.

June 23.

SIR,

I have received the favour of yours with the enclosed, which made up the Papers of the two last days. I cannot but look upon myself with great contempt and mortification, when I reflect that I have thrown away more hours than you have lived, though you so much excel me in everything for which I would live. Till I knew you, I thought it the privilege of angels only to be very knowing and very innocent. In the warmth of youth to be capable of such abstracted and virtuous reflexions (with a suitable life) as those with which you entertain yourself, is the utmost of human perfection and felicity. The greatest honour I can conceive done to another, is when an elder does reverence to a younger, though that younger is not distinguished above him by fortune. Your

contempt of pleasure, riches, and honour, will crown you with them all, and I wish you them, not for your own sake, but for the reason which only would make them eligible by yourself, the good of others.

I am, dearest youth,

Your friend and admirer,

NESTOR IRONSIDE.

*Thursday.*

Others have pointed out that the 'dearest youth' here addressed is probably Berkeley, and that this identification, if correct, carries with it the authorship of Nos. 88 and 89. Fraser in his 1871 edition (vol. iii, p. 183, n.) writes, 'See the second letter in *Guardian*, No. 90, which (if not to Mr. Deane Bartelett<sup>1</sup>) was probably addressed to Berkeley. Certainly the contents of the letter point to Berkeley. His philosophical detachment, his contempt for riches and honour, his zeal for the good of others, his learning, and his virtue and innocence—these were just those ingredients of his character which drew the wondering attention of the London wits. Bishop Atterbury on meeting Berkeley said of him, 'So much understanding, so much knowledge, so much innocence, and such humility, I did not think had been the portion of any but angels till I saw this gentleman'. Steele's words, 'the privilege of angels only to be very knowing and very innocent', are so like Atterbury's that the one eulogy is probably an echo of the other; and it would be a strange coincidence if the one referred to Berkeley and the other not. When we turn to the contents of the essays, we find them so much in Berkeley's manner, exhibiting his characteristic attitude to free-thinking, containing close parallels with his admitted essays in the *Guardian* and his other writings, that the probability that Berkeley wrote them amounts to moral certainty. Here are the parallels.

No. 88.

'ignorant and savage people', enlightened by the Gospel, compared with the ancient Greeks; cf. No. 62.

'If there be any of the free-thinkers who are not direct atheists . . .'; cf. 'a certain person in a mask, who was placed in the center . . . Atheism', No. 39, and Alc. i. 9.

Among the texts note 'in whom we live and move and have our being', which expresses the heart of Berkeley's metaphysic, and is quoted, Princ. 149 and in several other prominent positions in his writings, including the title page of the *Theory of Vision Vindicated*.

'the light of nature', cf. Pass. Obed. 12, Alc. vii. 27.

<sup>1</sup> Deane Bartelett is not named in *The Publisher to the Reader* as a contributor, but in the 1789 edition of the *Guardian* one paper (No. 130) is assigned to 'Mr. Deane Bartelett of Merton College', and it is there stated in a note that Steele quotes from the paper as coming from Mr. Bartelett.



Inter-dependence of natural and revealed religion, and the inconsistency of free-thinkers who would support natural religion, while discrediting revelation; cf. Alc. v. 9 ff., and, especially, 29, 'Whoever thinks highly of the one can never, with a consistency, think meanly of the other . . .'.

## No. 89.

'this introversion of my faculties'; for the term cf. CPB. 539 (544), and Alc. vi. 13, 'a tiresome introversion of his faculties'.

The joy of immortality, cf. No. 49 *ad fin.*, 'this comfortable truth', cf. 'in regard to us so comfortable', No. 27.

'the shady realms of Pluto', cf. No. 27.

'Eye hath not seen', cf. No. 27 and my note there; cf. 'this indefinite description' with 'this empty though emphatical description' in the sermon.

'raised to a higher pitch', cf. Nos. 27, 70, and in the sermon, 'when God has given the finishing stroke to our nature . . .'; 'condescension to our low way of thinking', etc., cf. in the sermon, 'which we narrow-sighted mortals wretchedly point out to ourselves . . .'. So Alc. vii. 13.

This scheme flows from the divine attributes (cf. No. 49), and corresponds with other parts of creation; cf. Nos. 27, 49.

'an affectation of singularity'; cf. 'the singularity and credulity of minute philosophers', Alc. vii. 21.

## \* No. 126. Wednesday, 5th August. MORAL ATTRACTION.

An analogical argument carried through at a high level of thought; for Berkeley's interest in the Newtonian doctrine of cohesion and attraction see Princ. 103 ff. Note that the essay avoids accepting gravitation as a physical force, and insists, as Berkeley always does, that the real force is 'the immediate operation of God'. The other part of the analogy, spiritual and moral attraction, cleverly sketched here, is elaborately worked out in *Alciphron Dial. I*, often in language that echoes this essay. There can be no doubt that the essay is by Berkeley.

'a certain correspondence of the parts', cf. 'a mutual connexion and correspondence of parts . . . the same union, order, and regularity in the moral world that we perceive to be in the natural', Alc. i. 16, and in the same passage the analogy is extended to the parallel between private and public good, and to 'the sympathy of pain and pleasure', cf. 'that sympathy in our nature whereby we feel the pains and joys of our fellow-creatures'.

'reciprocal attraction in the minds of men . . . intercourse with his species' ('not the result of education . . . but a principle originally engrafted . . .') cf. Princ. 147, 'He alone it is who . . . maintains that intercourse between spirits . . .'.

'They are drawn together into communities, clubs, families, friendships, and all the various species of society', *cf.* Alc. iii. 3, 'All rational beings are by nature social. They are drawn one towards another by natural affections. They unite and incorporate into families, clubs, parties, and commonwealths by mutual sympathy.'

The case for the Berkeleian authorship of the twelve essays, Nos. 27, 35, 39, 49, 55, 62, 70, 77, 83, 88, 89, 126, is thus established, it seems to me, beyond a doubt. I will give a brief summary of the foregoing evidence, appealing not at all to the son's list of his father's essays, and appealing very little to the great mass of detailed cross-references which I have accumulated in the above analysis.

We start from the publisher's published statement that Berkeley contributed 'many excellent Arguments in Honour of Religion and Virtue', and ask which of the essays are meant. I reply first that the group 27—35—39—49—55 have an incontestable claim; they all concern religion and virtue; they form a solid quintet; 27, 35 and 39 are linked by the M. Deslandes incident. The conclusion of 49 claims several papers already written about the free-thinkers, and links up with 55 by the promise of dealing with the free-thinker's contention that virtue should be its own reward, and forecasts other papers as well. No. 27 on the face of it is by a clergyman and a philosopher, and no one, I think, who knows Berkeley's *Commonplace Book* and his sermon on Life and Immortality could fail to be reminded of them by the concluding paragraph of this essay. Assign No. 27 to Berkeley, as we must, and the rest of the quintet goes with it. We have then to study the three, an inseparable trio, Nos. 70—77—83, and the pair Nos. 88—89, and we find that the trio and the pair connect up both with Berkeley's *Alciphron* and with the quintet of essays already assigned to him. Thus ten essays come on Berkeley's list, and in partial confirmation of the assignment we notice that almost certainly Steele himself in No. 90 assigns to Berkeley two of these (Nos. 88 and 89) with others unspecified. There remain only Nos. 62 and 126. These stand somewhat apart from the rest of the series; but they both take their place in the attack on the free-thinkers, and on careful examination of the parallels given above, both essays will be found to be linked up with essays admitted to be by Berkeley and with his other writings.

Are there any more essays in the *Guardian* which should be claimed for Berkeley? I do not think so. Here is a list of those which deal with 'religion and virtue' together with the names of the authors to whom they are attributed in the 1789 edition: Nos. 18 (Steele), 21 (Steele), 51 (Steele or Dr. Young), 56 (Dr. Thomas Parnell), 74 (Bishop William Beveridge), 75 (no assignment), 93 (Dr. William Wotton), 130 (Deane Bartelett of Merton College), 169 (Steele). In addition to No. 3, discussed above, the following

contain references to free-thinkers, Nos. 93, 130, 169. A few of these essays share with Berkeley's a philosophical approach to religion; but most of them contain passages which Berkeley could not have written, and they all lack those parallels with Berkeley's admitted essays and other writings such as I have pointed out in the foregoing analysis. No. 130 contains, I think, the nearest approach to Berkeley's style and manner, and I have sometimes wondered whether there could have been a confusion between the names which sound so similar, Deane Bartelett and Dean Berkeley; but on reflexion I cannot think that Berkeley would have written of the gentleman and the mechanic as this writer does. Berkeley would not identify the gentleman with the rational part, nor the mechanic with the animal part, and the quaint passage about ogling with the eye and flirting with the fan is not in his manner.

Nos. 101 and 104 are both assigned to Addison in the 1789 edition, but with the suggestion in a note that Addison was really only the publisher and that Berkeley wrote them. Moore's edition (1793) actually assigns 101 to Berkeley. The suggestion was due, no doubt, to the fact that Berkeley *later* sent home descriptions of various places he visited on the Continent; but here the suggestion is quite impossible; for as we now know from the Berkeley-Percival correspondence, Berkeley did not leave London for France till 25th October, 1713, and by that time the *Guardian* was defunct.

A. A. LUCE.

## V.—CRITICAL NOTICES.

*Mathematical Logic.* By WILLARD VAN ORMAN QUINE. London : G. Allen & Unwin, 1940. Pp. xiii + 348. 21s.

FORMAL logic has never quite recovered from the shock administered, some forty years ago, by the publication of the now notorious logical paradoxes. The short way with such paradoxes is also the least satisfactory. To use *ad hoc* restrictions upon admissible symbolism in such a way as to exclude the arguments known to entail absurd conclusions is a course of action requiring considerable technical dexterity but presenting no insuperable difficulties ; it has, however, all the disadvantages attending a policy of opportunism. For justification it must rely upon demonstrated success in evading the *known* paradoxes ; but it is always subject to the suspicion of concealing further contradictions as yet undiscovered. Should it be successful in achieving consistency, it would still be subject to the reproach of substituting prophylaxis for intelligibility. The merits of Russell's theory of types, in its original form, consisted in its derivation (at least in part) from principles that were both intelligible and independently persuasive ; its defects arose from the severity of some of its prohibitions, the redundancy of others, and the implausibility and lack of definition of the devices introduced to remove these evils.

Any system of formal logic aiming at comprehensiveness is still compelled to grapple with the unsolved problem, created by the discovery of the paradoxes, of combining intelligibility with self-consistency and technical adequacy. Professor Quine's book is a notable contribution to this enterprise. Like much of his previous writings, it proves to be largely concerned with the rehabilitation of the 'logistical thesis' that mathematics is reducible to logic ; his book is, therefore, the latest term in a series of researches which includes Peano's *Formulaire*, Frege's *Grundgesetze*, and the *Principia* of Whitehead and Russell. No contemporary work in this field can have the novelty of the classical researches ; Quine's book will, however, bear comparison with its distinguished predecessors in respect of precision and sustained excellence of presentation. With them, too, it has shared the ironical fate, reserved for inquiries in mathematical logic, of proving to harbour contradictions unsuspected at the time of writing. The two years elapsing between this book's publication and its receipt for review have been sufficient to bring to light and to confirm the presence in Quine's system of a radical internal contradiction, not to be circumvented by minor readjustments of detail but requiring rather a thorough-

going revision of some of the most characteristic features of his exposition.<sup>1</sup>

Only the malicious or the uninformed will draw hasty conclusions from this logical catastrophe; a substantial portion of Quine's logic remains unaffected, as we shall see, and the demonstrated limitations of the remainder themselves constitute a valuable aid to further progress in this disputed territory.

The vicissitudes so soon endured by Quine's system will, however, incline the reader to treat with some caution the claim to have combined "textbook and treatise . . . within the same covers" (preface, v). In spite of the pedagogic excellence of the exposition of many topics (of which instances will be given in the detailed analysis which follows) the selection and organization, both of subject-matter and symbolic machinery, tends to be determined by convenience for the derivation of the later, controversial sections rather than by considerations of ease in understanding, learning and use. The ideal textbook in mathematical logic would include extensive discussion of the intensional and modal logics, the use of proof theory (metalogic) to establish consistency and completeness, and the *application* of formal logic in connection with deductive systems (to name only three topics which receive scanty mention here). Even the "serious reader" for whom the book is intended (v), while he accepts the demands upon his serious attention arising from the fact that "rigor has not, in general, been consciously compromised in favor of perspicuity" (*ibid.*), may yet desire certain customary props and alleviations of his toil; the more liberal use of typographically distinct symbols (*e.g.* to differentiate symbols of different types) and some preliminary, informal, description of the essential 'point' of crucial proofs would augment perspicuity without detriment to rigour.

As an expository text this book will be most useful when treated in the light of a supplement to the good elementary accounts (say by Tarski or Cooley) now available. When so employed it will serve the purpose of inculcating, by precept and example, standards of clarity and precision which are, even in formal logic, more often pursued than achieved.

Such services to the non-specialist reader are most conspicuous in the first three chapters.

Chapter i, entitled "Statements", is an excellent brief account of the propositional calculus. This is well-trodden ground, but I do not know that it has been surveyed anywhere with more elegant

<sup>1</sup> The obstreperous paradox involved is that of the greatest ordinal, first published by Burali-Forti (1897). That this is derivable in Quine's system is proved formally by Professor Barkley Rosser in his paper, "The Burali-Forti Paradox," published in *The Journal of Symbolic Logic*, Vol. 7, pp. 1-17 (March, 1942). The proof has been certified as valid by Quine: cf. his "Element and Number", in the same journal (Vol. 6, pp. 135-149), which contains detailed proposals for the revision of the system of *Mathematical Logic*.

brevity. The various statement-connectives, having been introduced informally by illustrations from common usage, are defined in terms of truth-tables; it is shown that all truth-functional modes of composition can be defined by the sole use of denial, conjunction, and alternation; and these in turn are shown to be definable in terms of the single connective, 'neither—nor' (joint denial). From this point onwards, all expressions involving the familiar statement-connectives are construed as abbreviations for more complex statements constructed by means of joint denial alone. By using the truth-table test for tautology (progressive calculation of the truth value of a composite statement for all combinations of truth values of the components) it becomes possible to establish any required theorems without appeal to specific axioms. This mode of procedure is no less valid and incomparably more concise than the more usual derivation of theorems from a selected set of axioms.

The chief novelty in this section is the emphatic introduction and observance of the distinction between the use and mention of symbols. Consider the two sentences :

- (1) Quine is a logician.
- (2) 'Quine' consists of five letters.

In the first a personal name is *used* (in the subject place), whereby a person is *mentioned*; in the second the same personal name is *mentioned*, another name is used (in the subject place), and no reference is involved to any person. The first word of (1) is the name of a *person*, while the first word of (2) is the name of a *name* (of a person). All of which might be considered sufficiently obvious, once attention is directed to it, and scarcely deserving of further discussion; yet the standard use of quotation marks to construct the name of the expression to be found between the marks (or, to speak more exactly, the *omission* of the quotation marks in such cases) has proved a source of perennial confusion between levels of discourse.<sup>1</sup> Quine shows persuasively the importance (once stressed by Frege but unhappily neglected since) of scrupulous and accurate insertion of quotation marks. An important application follows in the distinction between the statement *connective* commonly denoted by '⊃' and the *relation* between statements indicated by the word 'implies' (28-33).

The distinction between names of objects and names of names, which the use of quotation marks is designed to signalise in a vivid and forcible manner, is carefully observed throughout the book; where general statements *about* expressions occur the distinction

<sup>1</sup> Illustrations of confusion of use and mention, persisting after Quine's explanation of the seemingly trivial point involved, are to be found in Professor Ushenko's "Dr. Quine's Theory of Truth-functions" (*The Journal of Philosophy*, Vol. 39, pp. 64-67). Cf. also Quine's "Reply to Professor Ushenko" which immediately follows the preceding.

is marked by reserving Greek letters for the designating symbols. Closely connected with the distinction between use and mention is that between a theorem and a metatheorem, the latter being, as the name suggests, a theorem *about* theorems, wherein symbols are mentioned and names of symbols used. Quine's procedure is to prove metatheorems wherever practicable. In this way he is able to establish a large number of related theorems, simultaneously and with a minimum of exposition.

In the proof of metatheorems a complication arises whenever it is desired to make general statements concerning statements or expressions constructed in a predetermined manner. If we should wish to refer to the denial of some undetermined statement,  $\phi$  (to take a simple instance), it is not strictly permissible to use quotation marks, thus: ' $\sim\phi$ '. For the complex symbol terminating the last sentence designates the *specific* symbol (composed of the negation sign and the Greek letter phi), depicted between the quotation marks. (The omission of the quotation marks would leave us with a sign compounded of a statement connective, the denial sign, and a variable *name*, i.e. a nonsensical complex.) To overcome this hitch Quine substitutes, in such cases, small right-angle brackets or "corners" for the commas. Thus the following sign, viz.: ' $\sim\phi$ ' designates the denial of whatever statement ' $\phi$ ' (ambiguously) designates. (It may be doubted, nevertheless, whether the additional typographical complexity of the device of "quasi-quotation" is worth the bother; once the point involved has been clearly understood there should be little danger in reverting to the more familiar use of the commas, leaving it to the context to decide whether they are to be interpreted in the sense of quasi-quotation or quotation proper.)

Any logician who finds the foregoing distinctions pedantic or pointless would probably benefit by reading the whole of this chapter; while the section on use and mention (23-33) might well be required reading for all philosophers.

In chapter ii we enter into quantification theory, i.e. what is more commonly styled the 'restricted' or first-order functional calculus. At this point the student usually begins to struggle with the meaning of 'variable' and 'propositional function' (not to speak of the gratuitous difficulties created by the ' $x$ -cap' notation in *Principia*). All such obfuscations are here effectively evaded by a nominalistic approach. Variables are taken to be selected letters, ' $x$ ', ' $y$ ', etc. (69); in place of the perplexing and ambiguous notion of propositional function we deal with "matrices", i.e. "*expressions* which would be statements if they contained names instead of variables".

This way of proceeding admittedly ignores any ontological questions about the status of variables and functions in which philosophers may continue to be interested; but the value of the nominalistic approach lies precisely in the demonstration of the mutual in-

dependence of the symbolic technique and the ontological issues. Quine's treatment provides us with the minimum explanations needed for the consistent and definite use of such terms as 'function' and 'variable' (itself no small achievement); further discussion of ontological underpinning, far from being prejudiced, is likely rather to be facilitated by the precise delimitation of the language for which it has to account.

In choosing a single primitive quantification idiom ('whatever  $x$  may be . . . ' or ' $(x)$ ') Quine follows standard practice; but in his rules for the scope of quantifiers we meet for the first time a technique which I shall call the *vacuous extrapolation of symbolism*. It consists, briefly, in enlarging the range of meaningful use of a familiar symbol in such a way as to avoid the consideration of exceptional cases; this procedure, familiar in mathematics (as in the convention that all lines in a plane shall "meet"), demands the re-interpretation of the symbol in the instances which its meaningful use has been extended to cover. Thus Quine allows us to write ' $(x)$  (Socrates is mortal)', i.e. to apply a quantifier to an expression from which the relevant variable is absent, on the understanding that ' $(x)(p)$ ' is then equivalent simply to ' $p$ ' (74). A propensity for such devices (i.e. a policy of uniformity of symbolism at the expense of simplicity of interpretation) is characteristic of Quine's system. In this instance the advantages of vacuous extrapolation are made very plain: we are able to unify the theory of statements and the theory of matrices with a minimum of reduplication.

The rest of this chapter calls for little comment. Derivation of theorems from a set of axioms follows familiar lines, except in so far as the formulæ involved may have any finite unspecified number of variables. By allowing the omission of any indication (via suffixes) of the number of variables concerned, it becomes possible to prove metatheorems of considerable generality; we pay for this, of course, though not at exorbitant rates, in mounting complexity of definitions and proofs. The discussion of the freedom and bondage of variables (76-80) will oblige the "serious reader" to justify his honorific title, and quite elementary theorems, requiring for their proof recursive arguments to take care of the indefinite number of variables involved, will exercise his powers of concentration and ensure that any increments of understanding are not unearned. (Cf. proof of \*III, p. 90.) This is a proper place to express appreciation and regret. In his notation for proofs, Quine uses ingenious abbreviatory devices (somewhat reminiscent of those favoured by Łukasiewicz) which effect great condensation of argument without loss of accuracy or clarity; but the explanations of the special notation introduced for this purpose are scattered throughout the text. In a new edition it would be a convenience to the reader to have the various explanatory discussions (now listed under "Proof notation" in the index) assembled in an appendix.



So far no reference has been made to specific statements or matrices. With chapter iii ("Terms"), and the introduction of the class-membership connective (' $\epsilon$ '), we begin to apply the preceding general theory to the study of classes. There are several points of considerable interest arising at this stage:—

(1) The distinction between classes and properties is virtually abolished—"It matters little whether we read ' $x\epsilon y$ ' as ' $x$  is a member of the class  $y$ ' or ' $x$  has the property  $y$ '" (120). In fact, the treatment being strictly *extensional* throughout, no independent treatment of properties (or propositional functions) is needed.

(2) In accordance with the policy of vacuous extrapolation we retain the symbolism ' $x\epsilon y$ ' even when  $y$  is not a class; in such a case ' $x\epsilon y$ ' is interpreted to mean ' $x = y$ ' (122).

(3) The expression ' $x\epsilon x$ ' is not syntactically inadmissible, though it may prove to be a false statement; indeed self-membership is the basis of such distinction as is to be found in this system between "individuals" and classes (135-136).

The second and third of these points will warn the reader not to expect any theory of types in the form proposed in *Principia*. It will be recalled that if we suppose the so-called 'ramified' theory of types (i.e. the portions concerned with the discrimination of 'orders' within the same type) to be abandoned on the grounds urged by Chwistek and Ramsey, we are left, in Russell's procedure, with what amounts to a series of restrictions upon the kind of condition ("matrix" in Quine's terminology) which the abstracting idiom, 'the class of all entities  $x$  such that . . .', is allowed to precede.<sup>1</sup> Only such matrices as conform to the (simplified) theory of types are regarded as *meaningful*; but we are always allowed to form a class (i.e. "to prefix the abstracting idiom", in Quine's phrase) consisting of *all* the entities satisfying a matrix which passes the type tests.

An alternative way of dealing with the paradoxes (first adopted by Zermelo) is to allow a maximum of freedom in the construction of matrices while applying restrictive limitations to the entities covered by the abstractive idiom; i.e. we restrict the phrase, 'the class of all entities  $x$  such that . . .', rather than the symbol which it precedes. "Under this procedure the realm of entities which can be formed into classes is narrowed somewhat, by deciding that certain classes are incapable of being members of classes" (130).

Thus we do not now claim that there are no such entities as the class of all classes which are not members of themselves (to take a crucial instance); we say rather that such wild classes *may* prove incapable of being members of classes. Since, as we have seen,

<sup>1</sup> The inconveniences of the theory of types are well summarized in Quine's paper on "Whitehead and the Rise of Modern Logic", contained in the composite volume entitled *The Philosophy of Alfred North Whitehead*. The whole paper throws much light on the work here reviewed, and might well be read in conjunction with it.

no distinction is made between class-membership and the possession of properties, we have to suppose that certain entities may *exist without having properties*. Philosophical criticism of Quine's system will find this consequence at once the most interesting and the hardest to accept.

Such proscription of membership has been previously used in the axiomatic set theories of von Neumann and Bernays (*cf.* the historical remarks on p. 165); Quine's proposal differs from these earlier constructions primarily in the more liberal admission of membership-eligible entities and the corresponding gain in manipulative freedom thereby achieved. His objective is to be bound by a condition of so general a character that no explicit appeal to it will be necessary except in a small number of crucial proofs. Postponing further discussion of the specific character of the restrictive conditions proposed, let us consider first the consequences, in Quine's logic, of this surrogate for the theory of types.

With Quine, we may abbreviate 'membership-eligible entity' into 'element'. Quine's method requires that the abstraction idiom shall be amended to read 'the class of all *elements*  $x$  such that . . .'; similarly, 'the class of all classes that are not members of themselves' must be revised to read 'the class of all *elements* that are not members of themselves'. And now the argument leading to Russell's paradox becomes innocuous, establishing only that the class designated by the last phrase in quotation marks *is not itself an element* (131), a result not at all perplexing once we have committed ourselves to the view that *some* entities are not entitled to be members of classes.

This approach, then, appears to provide a promising way to circumvent the paradoxes; but everything will turn, of course, upon the precise conditions chosen to determine "elementhood". Here the crucial notion is that of "stratification", whose definition reminds us of Russell's theory of types in the simplified version: "Let us speak of a formula as *stratified* if it is possible to put numerals for its variables (the same numeral for all occurrences of the same variable) in such a way that ' $\epsilon$ ' comes to be flanked always by consecutive ascending numerals (' $nen + 1$ ')" (157). (The detailed application of the stratification test is too elaborate to be summarised here, *cf.* 319-324.)

There is, however, this crucial difference between the two methods: in Quine's system, stratification of a formula is merely a *sufficient* condition for denoting a membership-eligible entity (as provided for by the all-important axiom \*200, p. 160); we do not reject unstratified formulæ as meaningless, nor is it excluded that specific *unstratified* formulæ shall denote elements. Hence we may apply abstractive prefixes to formulæ without attention to their stratification, and the abstracts so obtained may be freely substituted for variables, indeed, submitted to any manipulative procedure without fear of generating nonsense. It is only when we wish to derive

theorems that we may occasionally need to ensure that clauses of elementhood are included; and the need to establish the elementhood of specific entities very seldom arises. Thus whereas in *Principia* the need to avoid a small number of objectionable trains of argument is made the occasion for wholesale elaboration of symbolism (the introduction of type-indices) whose redundancy is then made apparent by the use of "typical ambiguity", in Quine's system the attempt is made to apply the restrictions only at the point where they are needed to prevent disaster, i.e. to inhibit only certain types of *proof*. We regain the manipulative convenience of which Russell deprived us, and typical ambiguity, with its unsatisfactory notions of "seeing" results and establishing theorems "by analogy", disappears for good.

Lest this seem too rosy a prospect it is necessary to add that the price of these goods is additional complexity of definitions to guarantee elementhood of the entities concerned (cf. the definition of relation, D24, p. 200) and some readjustment of the intuitive notions which help all of us to manipulate a symbolism. But new definitions can be learnt; and one soon becomes familiar with the points of deviation from earlier systems (e.g. the fact that *V*, the "universal" class, now contains only those entities which are "elements") which might otherwise give trouble. The net gain in simplicity and coherence of the resulting system is very considerable; it is regrettable indeed that the contradictions subsequently discovered should have persuaded Quine that the leading idea of 'stratification' with its corollaries needs to be abandoned (see below).

I must pass more quickly over the remainder of this important chapter, noting only that contextual definitions are provided, in terms of identity and class-membership, of the various types of formulæ in which abstracts can occur.

In chapter iv ("Extended theory of classes") we begin to use the crucial stratification axiom (\*200). Among the results proved are the elementhood of the universal and null classes. Definition of unit classes and derivation of the usual theorems of the calculus of classes present no especial difficulties.

The point of departure of chapter v ("Relations") is the definition of a relation as a class of ordered pairs. Let  $x$ ,  $y$  be any two entities; let  $u$  be the class whose sole member is  $x$  and  $v$  the class whose only members are  $x$  and  $y$ ; let ' $(x; y)$ ' denote the class whose only members are  $u$  and  $v$ ; we take  $(x; y)$  to be the ordered pair constituted by  $x$  and  $y$ . This mode of approach, modelled on the work of Wiener and Kuratowski, allows a relation to be interpreted as a class (of classes of classes) and so obviates the need for independent derivation of the theory of relations as in *Principia*, the major theorems being easily deduced as special cases from the general theory of classes expounded in the preceding chapters. Also discussed in this chapter is the definition of functions.

Apart from minor improvements in points of detail the definitions introduced and the theorems proved in this chapter follow the lines of *Principia*.

Chapter vi brings us to the definitions of *zero, one, natural number* and *finite*. The most striking result yielded in this part is the proof that *V* (the class of all *elements*) is infinite (theorem †673, p. 251). Thus no supplementary axiom of infinity is required (though presumably an axiom of choice would be needed in the further elaboration of the theory of sets). The familiar identities of numerical algebra are rigorously demonstrated, but the definition of ratios and the usual methods for defining real numbers, limits, etc., are merely sketched.

The last chapter ("Syntax") is a formalization of the meta-mathematical or syntactical symbolism used throughout the earlier parts of the book. At this stage one appreciates the technical convenience, for syntactical studies, of the various notations previously introduced; Quine is able to formalize his syntax with considerably less labour than is required in the similar accounts of his predecessors. (I am speaking here of comparatively recent studies, of course; the syntax of *Principia* is unformalized.) A restricted portion of the syntax (that which omits membership) is distinguished by the label "protosyntax"; within this meagre syntactical system it is shown to be possible to establish a theorem of incompleteness (closely parallel to Goedel's celebrated result); and this appears to be a somewhat sharper result than any incompleteness theorem previously demonstrated.

The whole of this concluding section on syntax is likely to be of somewhat restricted and transient interest; the field of syntactical studies is still so much in process of preliminary organization that it is unlikely that the specific instrumental devices here employed by Quine will achieve permanent recognition. A final comment is worth reproducing as an indication of the modesty of the programme which circumstances have compelled formal logicians to accept: "... a notion of theorem capable of exhausting those logical formulæ which are true and excluding those which are false will be definable only in a medium so rich and complex as not to admit of a model anywhere in the reaches of logic and the derivative body of mathematics. An exhaustive formulation of logical truth remains a worthy undertaking; but an exhaustive formulation which carries general recognizability [*i.e.* acceptability?] with it, even of the most tenuous sort, is not to be aspired to" (316).

It should be added that the first six chapters form an independent and self-contained whole, to which the discussion of syntax may be regarded as an appendix, to be skipped at pleasure without detriment to the remainder.

There remain an appendix on the technique of establishing stratification; useful lists of the definitions, theorems and meta-theorems used in the text; a bibliography of works to which reference has been made; and adequate indexes.

Viewed as a whole, the system of logic explained in this book comes nearer than any previous attempt to conforming with the regulative ideals of the mathematical logician. By using the reductive discoveries of Nicod and Sheffer in the propositional calculus and of Wiener-Kuratowski in the theory of relations, every statement expressed in purely mathematical terms becomes an abbreviation for a logical statement, using only the three logical primitives of class-membership, joint denial and quantification (with the associated variables) (129). And the axioms required for the *proof* of the theorems in the system are few in number and not intrinsically unpalatable. This advance in unification and deductive elegance is not achieved at the expense of rigour; while the striking nature of the gain in the conciseness of the whole may be verified by any reader who will compare the length of this book with that of the corresponding sections in *Principia*.

How far is this satisfactory outcome disturbed by Rosser's discovery that the Burali-Forti paradox of the greatest ordinal is deducible within Quine's system? In order to answer this we have to remember that the paradox in question essentially depends upon the use of 'impredicative formulæ' (roughly speaking: the use of relations defined by the use of formulæ requiring the quantification of relations); the theory of types, by ensuring at critical points in the argument an increase in types, barred the appearance of the contradiction, but it appears upon examination that Quine's weaker stratification device fails to do the same.<sup>1</sup>

One way out of this difficulty (though not the one discussed or adopted by Quine in the paper to which reference has already been made in the first footnote of this review) would presumably be to scrutinize Rosser's derivation of the paradox in the hope of determining what modification of the stratification procedure would be sufficient and no more than sufficient to break down the proof. This, if it should prove successful, would retain the general merits of Quine's treatment, and would involve, we might hope, mere supplementation of the stratification conditions. Suppose it were found that, besides being stratified, a formula would need to satisfy some further condition,  $\kappa$  say, before qualifying as the designation of an "element"; we should then need simply to amend the stratification axiom (\*200, p. 162) by substituting for the first clause the words "If  $\phi$  is stratified and satisfies  $\kappa$  . . .". Relatively minor adjustments, to ensure that all formulæ whose stratification is demanded in the proofs of the theorems included in this book also satisfy the further condition  $\kappa$ , would be all the correction needed.

The objections to this proposal are threefold: our ignorance of

<sup>1</sup> The stratification device occurs in a somewhat different way in an earlier system of Quine's, not known to be inconsistent, where it figures as a condition of existence rather than of mere elementhood. Cf. his "New Foundations for Mathematical Logic", *The American Mathematical Monthly*, Vol. 44, pp. 70-80.

the existence or character of any such supplementary condition ; the increasing complexity entailed by the kind of ramified stratification which is the best we could hope to achieve ; and the absence of any guarantee that the safeguarding device introduced specifically to neutralize the paradox of the greatest ordinal would be sufficient safeguard against the resurgence of brave new contradictions.

Such considerations as these may have led Quine (in his paper on "Element and Number") to take the alternative measures of rehabilitation which he now recommends. The single axiom \*200, admitting to elementhood all entities designated by a stratified formula, is now to be replaced by a series of weak axioms of elementhood. By requiring, (a), that a unit class shall always be an element, and, (b), that a simple function of every two classes which are elements (*viz.* the logical product of their complements) shall always be an element, it is found possible to derive all the theorems of the first five chapters (*i.e.* the whole of elementary logic inclusive of relation theory). The theory of arithmetic requires the addition of a further, somewhat more complex axiom (which appears as the theorem †610, p. 238, in the unrevised version). With this meagre battery of axioms in place of \*200, it is possible to prove all the theorems of this book, and even a substantial portion of the theory of ordinal numbers not therein included. The remedial treatment requires also a number of changes in proofs (and of course the deletion of all reference to stratification), as explained in detail in the paper already cited.

This policy is frankly one of piecemeal postulation or, as Quine calls it in a striking phrase, that of guaranteeing "elementhood in dribblets". In its opportunistic provision of such axioms as will in fact guarantee the deduction of progressively more extensive departments of logic and mathematics, while abandoning the purpose of providing a unified and rationally coherent treatment of the whole field, it constitutes a reversion to the formalist programme of Hilbert and his school. Russell's caustic comment, made long ago, about the advantages of such a policy over that of "honest toil", while showing some lack of appreciation of the strenuous nature of a life of crime, still retains its point. If piecemeal postulation is the best that the mathematical logician can do, the need for metamathematical investigations into the consistency of the amended system will be urgent ; once we abandon recourse to our intuitions and "resort to myth making" (as Quine in another context has suggested we must), the need to bolster our myths by some justification other than the pragmatic consideration of convenience for the technical needs of mathematics and the sciences becomes imperative.<sup>1</sup>

<sup>1</sup> A proof of the consistency of Quine's unamended system, when curtailed by the omission of the stratification axiom, is already available. Cf. Barkley Rosser, "The Independence of Quine's Axioms \*200 and \*201", *The Journal of Symbolic Logic*, Vol. 6, pp. 96-97. (This covers only the elementary, non-controversial portions of the system, of course.)

I have raised questions of the philosophy of logic which Quine's book does not undertake to discuss. But it is impossible to end without wondering how much competence it is desirable for the philosopher to have in this difficult and still controversial field. To the wider concerns of philosophical inquiry the relevance of such investigations is not immediately obvious; and many philosophers may be content to echo Locke's pious maxim that "our business is not to know all things but those which concern our conduct" in an interpretation which might exclude extensive preoccupation with the technicalities of mathematical logic. But to the extent to which the subject is a direct outcome of the attempt to find a systematic and rational formulation of the standards and principles of discourse, it may claim an important place in the corpus of philosophical analysis; its technical complexities are intrinsic to the subject-matter rather than any sign of the perversity of its investigators.

This notice will have failed in its purpose if, through necessary attention to the imperfections of Quine's system, it should discourage the reader from a closer study of his work. Every section of this book provides evidence of rare skill, both in research and communication; it deserves to be read and read again by all who have a serious interest in mathematical logic.

All concerned in the production of the book must be congratulated on the admirable format of what must have constituted an exceptionally difficult piece of printing.

MAX BLACK.

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*Science and Ethics*. By C. H. WADDINGTON AND OTHERS. London: G. Allen & Unwin, 1942. Pp. 144. 7s. 6d.

THE first 11 pages are an essay by Dr. Waddington, the next 36 pages are comments by thirteen authors with replies by Dr. Waddington, the next 38 pages, entitled "Some psychological considerations", are by Dr. Karin Stephen, Mrs. Klein, Miss Rothschild and Dr. Waddington, the next 10 pages are a correspondence between Professor Dingle and Dr. Waddington, the next 12 pages, entitled a Marxist critique, contain Professor Bernal's criticism and Dr. Waddington's reply, the next 15 pages contain final comments by the Bishop of Birmingham, the Dean of St. Paul's, Professor Chauncey D. Leake and Dr. Waddington. Finally, there are some notes on the contributors and an index.

Dr. Waddington has three purposes: (1) to contend that ethical statements, such as "You oughtn't to kill", are empirical generalizations like "You oughtn't to take less than 7 mg. of vitamin C daily"; (2) to emphasize and increase our grasp of the connexion between ethics and psycho-analysis; (3) to contend that "in the world as a whole, the real good cannot be other than that which



has been effective, namely that which has been exemplified in the course of evolution" (p. 18).

(1) In saying that ethical statements are statements and statements of scientific fact Dr. Waddington opposes, first, those who say that ethical statements are exclamations and/or exhortations, and second, those who say that they are self-evident truths stating a necessary synthetic connexion between, on the one hand, such things as cruelty, content, and exhilaration, and on the other hand, goodness and badness, which are detected by a moral sense or intuition. He less explicitly opposes the latter because, finding it impossible to produce a formal definition of goodness in terms of naturalistic characters, he cannot venture to say definitely that the good or evil of a state of affairs is a matter of *what* that state of affairs is and of how we feel and shall feel towards it. This combination of a transcendental ethical ontology with his positivistic epistemology work havoc. (See Broad's notes on the symposium between Waddington and Ewing, *Proc. Aris. Soc.*, 28 May, 1942.) I shall assume that if Waddington had been acquainted with recent work on how one thing may be a matter of other things without being formally definable in terms of them then he would have constantly said what he sometimes finds himself obliged to say, namely, that goodness is a matter of natural characters.

Though less dogmatic than Professor Bernal in his little sermon (p. 114) Dr. Waddington is almost equally innocent. To deal with the intuitionist doctrine of ethics it is necessary not only to make fun of it but also to remove the difficulties and admit the limitations of positivistic accounts. Dr. Waddington in rejecting the rhetorical doctrine and the analytic doctrine (Dr. Karin Stephen, p. 79, lines 8 and 9) makes this impossible. Yet Sidgwick, Moore and others cannot be ignored. No doubt their thinking, like that of poor Broad, is vitiated by the fact that their "concepts are non-developmental" (p. 51) or are "anthropomorphic abstractions such as Will, Conscience or Desire" (Bernal, p. 120). It would be kind, however, to explain a little *how* it is vitiated so that those of us who are still so anthropomorphic about men may the sooner go to where we are going, or should I say, the sooner reach that realistic ethos and superior social environment towards which the dynamic processes of evolutionary change are even now inevitably conducting us.

If someone says of a baby, "He's his father to a T", while another says, "No, no, he's his mother all over", then one expects to find each right in some ways and to find that some of the ways in which the one is right make the other that far wrong, and that anyway the baby is quite a lot just like himself and not like his parents. It is the same when someone tells me that a giant panda is just a little bear while another says, "No, he just a big guinea pig", while a third says, "No, he isn't anything but himself, the living essence of all cuddly toys". What makes one think well or ill of



these paradoxes, hyperboles, exaggerations, simplifications, is an intricate tale. Certainly, it isn't only a matter of the degree of likeness between the things concerned. Even if the baby is much more like his father than his mother, if the likeness to the mother can be revealed to me by suitable talk then I may think better of "No, it's his mother he's so like", just because this likeness doesn't leap to the eye but is subtle and profound.

"Ethical statements aren't statements but are commands, exhortations, exclamations"<sup>1</sup> is false but profound and, till recently, novel, though *strongly* hinted at by Hume. "Ethical statements are statements, statements of fact" is of course true. They are statements of fact about what is good and right, *i.e.* about ethical fact. And mathematical statements are statements about mathematical fact. And both sorts of fact can be discovered. But to say all this is platitudinous and useless except in opposition, *i.e.* as corrective to falsehoods such as, "They are exclamations". For, as always in metaphysics, the difficulty lies *within* the expressions 'statement', 'fact', 'true', 'false', 'discover'.<sup>2</sup> It is not for nothing that we use the same words in ethical and æsthetic and mathematical procedure as we use in scientific procedure. But "Ethical statements are statements" is a platitude and reveals nothing, for it reminds us only of a likeness our notation already emphasizes. It can be useful only in opposition to a paradox, *i.e.* in the way "A man is a man and neither a monk nor a monkey" (with apologies to Forster) is useful.

In contrast to this, "Ethical statements are statements of scientific fact, and in particular of psychological fact" is not a platitude and boring, but false and though not novel can still be illuminating if followed up in a novel way. I don't feel that we can say that Dr. Waddington does this, but he *does* make a suggestion as to how it could be done.

(2) Dr. Waddington is anxious to increase our grasp of and emphasize the connexion between ethics and psycho-analysis. It is the speaking out of this idea and the help which Dr. Stephen and Mrs. Klein give him in doing so which make this little book worth something, so it seems to me. The idea is still new though it has been put forward before to some extent, *e.g.* by H. V. Dicks when in *Clinical Studies in Psychopathology* (p. 109) he writes, "Clearly the union of the opposites has been set as the highest goal of human achievement . . . a task to be fulfilled by the individual within himself—a process of psychological growth and unification—the resolution of conflict, to give it its modern name. The discovery of oneself, the finding of the centre from which we cannot err, of the 'still, small voice' of the 'Golden Flower', . . . of the thousand-

<sup>1</sup> See "Ethical Words and Ethical Facts", Duncan-Jones, *MIND*, 1933.

<sup>2</sup> We can all remember those O so valid proofs that mathematical statements aren't commands nor rules of grammar because it is *true* that  $375 \times 2$  equals 750 while neither commands nor rules *can* be true.

petalled lotus, etc., etc., by whatever name this precious self-realization and acceptance has been called—this is nothing less than the aim of psychotherapy, within the limits of the patient's powers."

This seems to me to be in a certain very important respect a better account of the connexion between ethics and analysis than Dr. Waddington's. On page 10 he writes, "the psycho-analytical discoveries, which are concerned with the development of the ethical systems of individuals, are the most profitable basis from which to begin an examination of the scientific basis of ethics". This suggests that psycho-analysts discover generalizations and that then from these ethics may be deduced.<sup>1</sup> This misrepresents the psycho-analyst who, like a mechanic or a teacher of dancing, is as much an artist as a scientist. And also it is the discoveries in analysis by the analysed rather than the discoveries from analysis by the analyst that are most importantly connected with ethics. And, finally, the "discoveries" in analysis like discoveries of beauty in pictures or music are as much alterations in oneself and one's object as discoveries.

An account such as Dicks' suggests how a man in the course of analysis may suddenly say to himself, "Why, this *is* ethics. I have been reading about it all my life, and sometimes when a difficult decision has been called for I have been forced to do ethics, but often the decision has been a matter of having to act one way or another—not a settling of things in myself."

Of course working through an analysis is *not* doing ethics. But just as besides doing logic in the sense of setting out logical rules there is logical practice from which the rules are extracted as mnemonics, so besides ethics there is ethical practice from which, *much* less successfully, ethical rules can be extracted if that kind of thing amuses you. Now it is not so wrong to say that part of analysis is ethical practice. By "logical practice" I mean estimating the value of arguments, offering and accepting arguments and sifting our reactions to arguments. By "ethical practice" I mean accepting and refusing persons, acts and feelings, and the sifting of those acceptances and refusals. I mean the asking and answering of such questions as, "*Is Jack such a blackguard?*", "*Ought I to have done that?*", "*Is it horrible of me to feel like I do when . . .?*" This is what Hume called, significantly, our decisions of praise or blame.<sup>2</sup>

But even if we say that it is this which is done in analysis we shall still be apt to mislead. For just as in deciding whether a picture is good and even in deciding whether a show is funny we don't merely look again at the picture and sift *our* reaction to it but *also* count the reactions of *others*, especially Clive Bell, so in deciding whether it would be right to do so and so and whether it was right

<sup>1</sup> Another *different* but still intellectualistic bias is to be seen in Professor Stebbing's *Ideals and Illusions*.

<sup>2</sup> *The Enquiry*, Appendix A.

to do so and so we count the reactions of others. If, say what we will and say what they will, they differ from us still, then we say either that they use words differently or, in different circumstances, that though it is funny to us, or lovely to us, it isn't to them. And but for the extra regularity of nature in the matter of sweetness and redness and hotness and roundness we should do the same for them and say, "It's red to us but not to others", or "It's sweet to children but not to adults". And just as the redness, the real objective redness, of a red flag is a matter of its redness to nearly everybody to-day and also to-morrow unless it's been dipped in ink so is the beauty of a face, the niceness of a person, and the rightness of an act, a matter not only of how they seem to oneself but also of how they seem to others, and not only now but also when the bands stop playing.

To sum up in jargon: Real redness is constructed from redness to A and redness to B and redness to C and, etc. And redness to A is constructed from seeming now red to A, still seeming, *e.g.* on closer inspection, red to A. Likewise satisfactoriness is constructed from satisfactoriness to A, to B, to C. And really satisfactory to A is constructed from seems satisfactory to A, still seems satisfactory to A after listening to it again, or even now that he is sober, or, etc. Likewise rightness is constructed from really seems right to A, to B, etc., and really seems right to A is constructed from seems right to A at first blush, still seems right to A after review, comparison, etc. It is with the business of the transition from "seems for the moment acceptable and right to A" to "seems really right and acceptable to A" that occurs in analysis.

I remember how after I had been going to an analyst for about a month I was walking home and thinking over the sort of thing I had been doing and I said with surprise, "Why, this *is* ethics". It was because what I had been doing struck me as being just more of the sort of thing I should call thinking a thing over to see whether it seems right to me, whether I can "accept myself" if I do it. And yet what I had been doing could also be described as going into how I felt towards doing this with those consequences or that with those other consequences. To say that right is a matter of what at infinity still seems right to everybody and that what seems right to so and so is a matter of what he finally feels, is not to make right more subjective than red or round—(though it *is* more subjective). But it is a naturalistic and anti-transcendental *metaphysic* of ethics, *i.e.* ultimate description of ethical activity. It is opposed to the type of talk in, *e.g.* *Principia Ethica*, which suggests that goodness is related to those natural characters which make a thing good and our feelings to them like the power of a horse is related to those structural characters which cause that power and our feelings to them. The naturalistic type of talk suggests that on the contrary goodness is related to stopping on the way from Damascus<sup>1</sup> and

<sup>1</sup> Karin Stephen, p. 79.

cups of water and so on and our reactions to these, like the grace of a dancer to her movements and our feelings for these. Her grace *is* a matter of the patterns she gives to our eyes and the lift she gives to our hearts. So there is no problem of how we know she's graceful.

All this isn't new. Mill said that the desirable is the desired and he didn't mean all the nonsense he's been said to mean. He didn't mean that the desirable is what happens to be consciously desired at the moment. He meant that it is what is really desired. What is really desired is what is desired when *all* our inclinations towards it are faced and not some ignored, including desires not to have a desire for such a thing; in other words, our desire for X is a real desire when all our desires for all that is for us in X have been "owned" and "sifted". What does this mean?

In ethical effort people take note of the voice or prick of conscience—of the immediate response, "O no, mustn't do that". But they do not always take this as final. They say, "But there's no harm in it really, it's only my puritanical conscience", and a small Dionysian voice grows louder, "It's foolish, but it's fun". They join in the frolic of the Restoration. And then they turn again and say, "The Puritans had something after all", and take to driving in a victoria round Balmoral—only to leave it for a faster car and the dancing 'twenties and so on. Oscillation in deciding between philosophical doctrines goes hopelessly on until one gives up suppressing conflicting voices and lets them *all* speak their fill. Only then can we modify and reconcile them. It is so with other things.

The sifting of one's reaction towards doing such and such a thing having such and such consequences is like the sifting of one's reaction to a picture or an argument, or one's reaction (non-ethical) to a person. Professor Joad recently said to me, "How much of our reaction to these pictures is nostalgia for France?" and again one may say, "How much of my reaction to that man's argument was rhetorical or due to flattery, how much logical?" In this way one may say, "How much of my reaction against a 'blind' on a Saturday night is due to my puritanical upbringing", or "How much is it due to my father's being such a drunken misery-maker every Saturday night". And saying this alters the reaction. True, one says, "I thought I disapproved a Saturday 'blind' but I didn't really", but one might equally well say "I used to disapprove a Saturday 'blind' until I connected my reaction to Bill's 'blind' to-night with my reaction to all father's 'blinds' when I was three". And there is much in analysis which isn't different in kind from this. It is still connecting. It is more extensive and it is finding, grasping, how much the past is hidden in the present. And this is best done not so much with what Proust calls the "intellectual memory" which he says "preserves nothing of the past itself", but in the way in which he found hidden in a madeleine and a cup of tea all those Sunday mornings and the streets and

gardens of Combray, in the way he inhaled "through the noise of falling rain, the odour of invisible and persistent lilac trees" from the 'Méséglise way'. It wasn't *dugouts* that River's patient feared, or, if you like, it was dugouts, only in them was hidden for him that frightening and shameful moment when having just pawned the watch his mother and father had given him he found himself caught in a narrow passage by that figure of vengeance, a big, silent dog, straight from the landscapes of the surrealists.

Though it is especially the past it is not only the past that is hidden in a reaction to this or that now. "Her voice is in the falling rain" even if she is still alive and even present. In a different way the future comes in too.

"Unborn To-morrow and dead Yesterday,  
Who cares about them if To-day be sweet?"

The question would not have been asked if the answer hadn't been "Everybody". Nor would it have been asked had not everybody sometimes wished the answer to be "Nobody". Is it that to get a real response to the present we need something that will "clear to-day of past regrets and future fears"? "In vino veritas" they say. "But the pubs mostly close at 10. Besides though the Méséglise and Guermantes 'ways' leave us exposed, in later life, to much disillusionment and even to many mistakes, . . . by the same qualities, and by their persistence in those of my impressions, to-day, to which they can find an attachment, the two 'ways' give to those impressions a foundation, depth, a dimension lacking from the rest". And in sifting one's reactions one may at first reject as infantile and compulsive what in a re-sifting one may accept though in a different degree and different way perhaps. "How much is this nostalgia for France?" we may ask, and looking again at the picture see it rather differently, and then turning on ourselves once more we may say "And why not nostalgia? Is it all to be rejected? Isn't the Primavera what it is, in part because in the face of the Flora are so many Springs?" It is not the mere fact that a reaction comes out of the past that makes it tyrannous—it depends on what past it comes from and what welcome it receives.

Ethical effort is the weighing of considerations and, as Aristotle said, there are no rules which will enable us to avoid this effort. The trouble is we are very apt to be dissatisfied with our weighing, the weights too often and too much change with every re-weighing. It is not that we want the weights never to change. And it is not so much abrupt and even frequent change that is the trouble—it is that oscillation which finds expression in "I don't know *what* I really want" which is so distressing. It's all very well for the vocalist to advise

"Experiment  
And it will lead you to the light."

No doubt if you wish your child to become a connoisseur in tea you will encourage him to try lots of teas. But then tea is cheap while murder, adultery and robbery are not. Besides that way, namely, seeing life, isn't adequate even with tea, and many men "experienced with horses" even when industrious and well intentioned are very dangerous. With your tea drinking child you will do your best in choosing the order in which he drinks the teas just as you would over the order in which he "looks at" horses, pictures and arguments in order that he may develop with respect to these from the stage at which all are marvellous or all a bore to a stage in which he has greater discrimination. And, unfortunately, he may easily come to hate them all more and more like Mrs. Arnot Robinson, came to hate ships, or become unable to like anything except Bach and Mozart or, worst of all, come to have a strong love *and* hate for all. This last is best avoided and best resolved by increasing his discrimination not so much of the objects to which he reacts as of his reactions to the objects. The mother says, "How would *you* like it?" *i.e.* "How much is your complaisance due to the fact that it's *you* who are pulling the cat's tail and not vice versa?" And in this she is *not* merely putting something into the child but *bringing out* the uneasiness which lurks in *him* just as it did when biting her breast he laid waste his world and with it himself.

There are other ways in which analysis is connected with ethics. For example, analysis isn't concerned only with what one really approves, really accepts in oneself, but also with what one really wants, fears, loves, hates. In deciding what to do, one of the things necessary is, of course, to estimate what consequences will follow and primarily what consequences in the way of how people, including oneself, will really feel, and that *not* in the way of approval or disapproval.

(3) About the connexion between what is good and what has been exemplified in the course of evolution. I heartily agree with Dr. Waddington's critics that most of what he says won't do at all and involves old muddles. But I would like to point out that on page 39 he says he means "if the ethical system is to be derived from the nature of the experimental . . . one of the most important data is the scientifically ascertained course of evolution". If it were not for the words "scientifically ascertained" this might easily mean "*One* of the criteria as to what a man or people really want is what they tend to get hold of in the end" and then it would be right though not easily applicable except in simple matters like wine and cars.

Finally, do we value Socrates (p. 38), Van Gogh (p. 90), because of their contribution to the progress of mankind? Not on your life. Social progress be damned. It's the picture that counts.

JOHN WISDOM.

## VI.—NEW BOOKS.

*Essays on the Intellectual Powers of Man.* By THOMAS REID, edited and abridged by A. D. WOOLEY, M.A. London: Macmillan, 1941. Pp. xl + 456. 21s. net.

THE editor in his preface apologises "for any signs of carelessness" which appear in this new edition of Reid's *Essays* and explains that the work was completed in "the far from academic surroundings of barrack-room life". The apology, however, is unnecessary since there are few signs of carelessness of any kind. The work is competently done, the text is well arranged and free from misprints, the editor's introduction and his footnotes are helpful and, lastly, the index gives the reader just the guidance he is likely to require.

Reid wrote two works on the theory of knowledge, the *Inquiry into the Human Mind on the Principles of Common Sense* (1764) and the *Essays on the Intellectual Powers of Man* (1785). Of the two the commonly accepted view is that the former is the more valuable in its philosophical discussion. At the same time it is very restricted in its scope, being confined for the most part to the problem of sense-perception, and no doubt it is for this reason that the editor chose the second of these works for re-presentation to the modern reader. It is to be hoped that an edition of the *Inquiry* will follow.

A new edition of Reid's *Essays* is to be warmly welcomed, for whilst Reid is not as important a figure as, for instance, Locke or Hume, his criticisms of these and other philosophers are helpful and much of what he says is relevant to our present problems. Moreover, it is only too true that the format of Hamilton's edition of Reid's *Collected Writings* (1846) is, as Mr. Woolley says, an "effective deterrent" to the study of Reid. This new edition in contrast has been well printed and is restful to the eyes. Hamilton's edition, however, is still likely to be found useful even in respect to the *Essays*. For, in the first place, Mr. Woolley's edition is abridged, whilst Hamilton's is complete. Secondly, Hamilton's contains in its footnotes, and particularly in its 'supplementary dissertations', an extraordinary wealth of learning and erudition to which I would draw the reader's attention if he does not already happen to be acquainted with it. Mr. Woolley has wisely made good use of this source of information—without of course depending upon it alone—but there is still much water left in the well. In spite of its double columns and ugly format Hamilton's edition, it is clear, will have to remain on our shelves.

To turn to the new edition, I should have been glad if the editor had told us something of the text. Is his edition based on the 1785 text or on Hamilton's? Are there any differences between the various editions, or any textual problems of any sort? Again, are there any manuscript remains of Reid's? Perhaps Mr. Woolley does not think these questions important, but a paragraph or two on such matters would have been of interest.

This is an abridged edition. Reid is somewhat long-winded; in addition he spends much of his time summarising the philosophical doctrines of



writers that are best read in the original. For these and other reasons it is possible to make a good case for abridgment here, and on the whole Mr. Woosley has, I think, abridged skilfully. There may, however, be objection to all abridgment on the ground that in abridging one may unwittingly omit what from some point of view or other is important. If the decision to abridge is made, it is certainly incumbent on the editor to explain quite clearly what he is omitting, and I feel that the present editor sins in this respect. He nowhere gives us a list of the omissions made. In the table of contents the titles of missing chapters are given within square brackets. But there is nothing at the end of the book, for instance, to show that it is not in fact the end, but that a whole essay, *Of Taste*—a not unimportant contribution to æsthetic theory—has been omitted. Further, Mr. Woosley omits paragraphs within chapters. Sometimes he adds a footnote informing the reader of the omission, but sometimes he does not; nor does he use dots to show that a passage is being omitted. Thus, of the first twelve paragraphs as printed by Hamilton of Essay III, chapter 5, eleven are omitted in the new edition. The only sign of the omission is that the title of this chapter is printed *Mr. Locke's Account [of the Origin of our Ideas, and particularly] of the Idea of Duration*. Would it not have been better to have made quite clear how much is omitted and where the omission occurs?

The editor's introduction gives a brief sketch of Reid's life and then expounds and criticises, with one eye on present problems in philosophy, the main contributions which Reid made in the *Essays*, under the headings, sense-perception, memory, conception, language, and common-sense. I found this summary of Reid's main doctrines most helpful and, as far as I am able to judge, very sound. I shall comment on two points only, of perhaps minor interest.

It is rather surprising to find Mr. Woosley saying on page xi that the dualist theory of sense-perception which Reid was attacking is 'obviously false' and seems 'so clearly untrue that we can wonder how anyone could bring himself to hold it'. This seems rather strong language. We may have our doubts about the dualist theory, but is it right to say that it is 'obviously false'? Mr. Woosley perhaps has in mind the crude copying theory. He refers to the 'idea-theory of cognition' of Locke 'and the dualism to which . . . it leads'. Yet fundamentally, terminology apart, Locke's problem is surely ours, namely, how sensing helps us in any way to know what is as a physical object, if we suspect, as we have good grounds for suspecting, that sensing is no direct knowledge of a quality or qualities of that physical object. Reid himself for that matter nowhere puts forward a crude sensationalism. For though he holds that we do know that physical objects exist, he does not think of sensation as a direct cognition of the qualities of such objects. Sensation rather provides, in Reid's opinion, natural signs which 'suggest' the real qualities. But is not this theory a dualism of a kind? And is Reid's 'sign' wholly different from Locke's 'idea'? Reid's theory of sense-perception is, I believe, an advance on Locke's; but it clearly does not provide an adequate solution of Locke's problem. We are, in fact, still searching for that solution and are consequently not in a position to say that dualism is obviously false.

Another remark which seems to me doubtful is the one Mr. Woosley makes in discussing Reid's theory of conception: "Reid admits no distinction in kind between imagining and conceiving" (p. xxvii). Mr. Woosley himself modifies this on the next page. Even so its suggestion



seems to me misleading. For in spite of his inconsistencies, Reid's real position in respect to this question is, I feel, best revealed on page 300 of the present edition when in a discussion of the very point at issue, whether when we conceive we must have a concrete image before us, he comes to the conclusion that we need have no such image. Conceiving and imagining, his argument here implies, are different functions of the mind, differing apparently in kind. (Incidentally, in connection with the discussion on conception I also find it impossible to accept the footnote to page 352.)

One can safely congratulate the author on a very useful edition of the *Essays*. Its publication should stimulate interest in Reid.

R. I. AARON.

*Reflections by a Journeyman in Philosophy on the Movements of Thought and Practice in his Time.* By JOHN HENRY MUIRHEAD. Edited by John W. Harvey, Professor of Philosophy in the University of Leeds. London: Allen & Unwin, 1942. Pp. 215. 15s.

At the time of his death in May 1940, Muirhead, who had just entered on his eighty-sixth year, was easily the doyen of contemporary British philosophers. He preserved his unflagging activity to the last; a reference to the admirable bibliography compiled by Professor Jessop and appended to this volume shows him in his closing years still busy with his work as Editor of the *Library of Philosophy*, writing numerous articles in philosophical journals, and, only a few months before his death, giving the Herbert Spencer Lecture at Oxford. *Scribens est mortuus*; his final task, the autobiographical record before us, having been brought up to 1930, so that Professor Harvey, who has undertaken the pious obligation of revision and completion, had but to add a single, though regrettably abbreviated, concluding chapter. It is to be wished that he had allowed himself to be more expansive in his sketch of the closing years of one whose loveable temper and singular gift for friendship, together with his readiness to show interest and even sympathy with new currents of speculative thought, especially in the younger generation, won him universal respect and affection. In the course of his long life, Muirhead had met all who counted for anything in his subject, both in this country and in America, and the charm of his disposition and his generous hospitality ensured that these meetings not infrequently ripened into the intimacy of friendship. Herein lies the chief interest of these Memoirs. Muirhead's sketches of eminent men and women whom he had known, especially in the course of his varied experience as a University teacher in Glasgow, Oxford, London and (for a quarter of a century) in Birmingham, are drawn with vivacity and discrimination, and mirror his own delightful personality; those, for instance, of Joseph Chamberlain, Sir Oliver Lodge and Bishop Gore in the Birmingham chapters and among his philosophic associates, of Green and Bosanquet, the two thinkers who successively left the deepest impression on his own mind. Muirhead, if not a philosopher of high originality, was extraordinarily susceptible to the influence of any ideas that were in the air. Though his sympathies were centred to the end in the Idealist tradition that had its roots in Kant and Hegel, he had bound himself to no exclusive school, and we can even picture him, had his life been prolonged for a few more years, probing into the intricate mysteries of the logical Analysts, in the resolve to achieve an understanding of the problems that engrossed the interest of a generation very alien to his own. There was

nothing about him of the partisan or the controversialist; later in his life, in *Rule and End in Morals*, he published an excellent critical survey of the discussions which engaged for over twenty years the attention of Oxford moralists, on the respective claims to primacy of the concepts of duty and of good. His chosen field, of course, was that of Ethics, and his record in this book shows that he felt most at home in the atmosphere of the London Ethical Society, of which he was a founder and chief mainstay. It gave scope to his missionary ardour which had been somewhat stifled during his residence at Manchester New College. We catch echoes in his comments on that period of D'Israeli's well known *dictum*, "The Unitarians in religion are like the Utilitarians in politics; they have no imagination and imagination rules the world." Muirhead, true to the Platonic tradition, liked to blend speculative theory with the more practical tasks of social service. Again, he was a firm believer, from early days at Glasgow, in the need for wielding together science and the Humanities in an all-round Arts curriculum. He records in these pages his conviction that the Glasgow *Quadrivium* in the 'seventies of last century, comprising Classics, Philosophy, Mathematics and Physics offered a more effective Arts training alike for the business man, for a professional career, and for the academic specialist, than any other to be found in British or American Universities, the Oxford school of *Literae Humaniores* not excepted. With characteristic modesty he calls himself, on the title-page of this book, "a journeyman in philosophy." Certainly he was this, for none of his contemporaries laboured so hard to mediate between the great philosophers, ancient and modern, and the intelligent public; but he was much more than this. No mere journeyman could have produced, as did Muirhead when well over seventy years of age, two such important contributions to philosophy as *Coleridge as a Philosopher* (1930) and *The Platonic Tradition in Anglo-Saxon Philosophy* (1931), the last mentioned being not only the most elaborate of his published books, but the best thing he ever wrote.

W. G. DE BURGH.

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Received also :—

- The Philosophy of G. E. Moore.* Edited by Paul A. Schlipp (Library of Living Philosophers, Vol. IV), Evanston and Chicago, Northwestern University; London, Cambridge University Press, 1942, pp. xvi + 717, 30s.
- S. K. Langer, *Philosophy in a New Key: A Study in the Symbolism of Reason, Rite and Art*, Cambridge, Mass., Harvard Univ. Press; London, H. Milford, 1942, xiv + 313, 20s.
- R. Carnap, *Introduction to Semantics*, Cambridge, Mass., Harvard Univ. Press; London, H. Milford, 1942, pp. xii + 263, 20s.
- G. H. von Wright, *The Logical Problem of Induction* (Acta Philosophica Fennica, Fasc. III). Editio Societas Philosophica, Helsinki, 1941, pp. 258.
- E. Kapp, *Greek Foundations of Traditional Logic*, New York, Columbia Univ. Press; London, H. Milford, 1942, pp. viii + 95, 10s.
- K. R. Sreenivasa Iyengar, *The Metaphysics of Value* (Vol. I), University of Mysore, Mysore, 1942, pp. xxxi + 645.
- A. Farrer, *Finite and Infinite*, London, Dacre Press, 1943, pp. xii + 300, 20s.

- C. E. Raven, *Science, Religion, and the Future*, Cambridge University Press, 1943, pp. x + 125, 7s. 6d.
- Plato Arabus*. Vol. II: *Alfarabius de Platonis Philosophia*. Edited by F. Rosenthal and R. Walzer, London, The Warburg Institute, 1943, pp. xxii + 30 + 21, 15s.
- E. C. Mossner, *The Forgotten Hume*, New York, Columbia Univ. Press; London, H. Milford, 1943, pp. xv + 251, 20s.
- In Commemoration of William James, 1842-1942*, New York, Columbia Univ. Press; London, H. Milford, 1942, pp. xii + 234, 18s. 6d.
- M. G. White, *The Origin of Dewey's Instrumentalism*, New York, Columbia Univ. Press; London, H. Milford, 1943, pp. xv + 161, 15s. 6d.
- R. W. Pickford, *The Psychology of Cultural Change in Painting* (The British Journal of Psychology, Monograph Supplement No. 26), Cambridge University Press, 1943, pp. vi + 62, 8s. 6d.
- D. Curran and E. Guttman, *Psychological Medicine*, Edinburgh, E. & S. Livingstone, 1943, pp. viii + 188, 10s. 6d.
- J. T. MacCurdy, *The Structure of Morale*, Cambridge University Press, 1943, pp. vii + 224, 8s. 6d.
- A Study in Punishment* (English Studies in Criminal Science, Pamphlet Series). 1. *Introductory Essay*, by L. Radzinowicz and J. W. C. Turner; 2. *Punishment as viewed by the Philosopher*, by A. C. Ewing. Toronto, Canadian Bar Association; Cambridge, The Squire Law Library, pp. 32, 2s.
- G. Katona, *War without Inflation*, New York, Columbia Univ. Press; London, H. Milford, 1942, pp. x + 213, 16s. 6d.
- J. Murphy, *Lamps of Anthropology*, Manchester Univ. Press, 1943, pp. ix + 179, 7s. 6d.
- T. V. Smith, *Discipline for Democracy*, Chapel Hill, The University of North Carolina Press; London, H. Milford, 1942, pp. xiv + 137, 12s.
- R. E. Taylor, *No Royal Road: Lucia Pacioli and his Times*, Chapel Hill, The University of North Carolina Press; London, H. Milford, 1942, pp. ix + 445, 24s.
- F. M. R. Walsh, *Diseases of the Nervous System*, Edinburgh, E. & S. Livingstone, 1943, pp. xvi + 350, 15s.

## VII.—NOTES.

### MIND ASSOCIATION: ANNUAL MEETING.

The Annual Meeting of the Mind Association will be held this year in New College, Oxford, at 3 p.m. on Friday, 2nd July.

### MIND ASSOCIATION.

Those who wish to join the Association should communicate with the Hon. Secretary, Mrs. Kneale, Lady Margaret Hall, Oxford; or with the Hon. Treasurer, Mr. H. Sturt, 55 Park Town, Oxford, to whom the yearly subscription of sixteen shillings should be paid. Cheques should be made payable to the Mind Association, Westminster Bank, Oxford. Members may pay a Life Composition of £16 instead of the annual subscription.

Members resident in U.S.A. may pay the subscription (\$4) to the Hon. Assistant-Treasurer, Prof. B. Blanshard, Swarthmore College, Swarthmore, Pennsylvania.